THE LANCET.

VOL. I.] LONDON, SATURDAY, OCTOBER 20, 1838. [1838-39.

COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE IV.

Facts in physics illustrative of the circulation of the blood.—Absurdity of medical terms.—Inflammation and irritation.—Coagulation of the blood.—Effects produced when the blood is rendered too fluid.—Action of œnanthic ether on the blood.—Post-mortem appearances in poisoning with this substance.—Viscidity of the blood; effects of this property.—Action and effects of bloodletting.—Changes produced by increasing the viscidity of the blood.—Globules of the blood; their importance in the circulation.

GENTLEMEN:—At our first meeting I solicited your aid in the researches I have undertaken. Your assiduous attendance on these lectures, and the lively interest you evidently take in the novel questions I have brought before you, are therefore particularly gratifying. One of you has just given me some blood taken from a patient; this I will take care to have examined, and if they are worthy of notice, you shall be made acquainted with the results. I derive much satisfaction from this proof of interest in my labours; I beg its author will accept my thanks, and, at the same time, I would entreat each of you to supply me, from time to time, with specimens of the bleedings you may have occasion to perform, accompanied with an account of the patients on whom they were practised.

For my part, from the 1st of January next, I will cause what I call trial bleedings to be regularly practised in my wards. Exact notes of these shall be kept, as well as of the previous and subsequent condition of the patients. By these means, at the close of the session, we shall have accumulated a large number of facts and observations, which will materially assist us in carrying out the plans we have laid down.

You remember, without doubt, the various facts we passed in review at our last meeting. Those facts showed you the importance of a close acquaintance with the state and composition of the blood, generally, and with such morbid changes as it is liable to undergo. They proved, also, that those changes play an important part in the production of a host of diseases, the causes of which appeared an impenetrable mystery. We have but just entered on the study of the blood; and scarcely had we done so, when we were fortunate enough to discover a phenomenon that had previously escaped the notice of both natural philosophers and medical men. Yet that phenomenon is one indispensable for the persistence of life, inasmuch as without it the circulation of the blood must cease.

Heretofore the particular conditions, which enabled the blood to traverse tubes of scarcely a thousandth part of a millemetre, were unknown. In all probability physiologists considered the matter quite natural, and therefore troubled themselves little about it. We think differently, however, and shall, therefore, continue our inquiry into the properties that confer on the blood that marvellous power.

The questions we have so far treated of belong, in great measure, whatever may be said to the contrary, to the subjects of physics and hydraulics. You will find that, in elucidating them, we shall have no occasion to avail ourselves of the so-called contractility of the vessels, which, in reality, does not exist; and we shall be quite as independent of the species of *sensibility*, termed *insensible*, and of the entire collection of absurd terms by the help of which no few romances, doubtless very clever, but very empty too, have been got up about the capillary circulation.

There is a fact, in physics, remarkable for the excellent term of comparison which it serves to establish between the phenomena of the movement of the blood in our organs and the circulation of liquids in inert tubes. I allude to the enormous pressure which is required in order to make water pass through

a tube of very small diameter, while the blood traverses with ease the infinitely more minute tubes that abound in our tissues. There must be some particular conditions to facilitate its passage. What proves their existence is, that if certain alterations are effected in the composition of the blood it stops, undergoes morbid changes, becomes extravasated and decomposed, and produces the various disorders which pathologists have vainly attempted to explain by the words *inflammation* and *irritation*. What sense, in truth, is there in applying the words inflammation to our organs? Do our tissues really take fire? I confess I know of no example of such a phenomenon. When the blood rushes to a part in abundance, a certain rise of temperature, no doubt, occasionally follows; but it only reaches a few degrees above the normal standard of the organ, and never exceeds that of the blood in the left ventricle. To cause a real inflammation, the elevation of heat should be carried infinitely higher; besides, in many cases, also called inflammatory, there is a notable fall of temperature. Some of you, perhaps, Gentlemen, accustomed to regard with deference the precepts of the schools, will look on this dislike to their incorrect nomenclature as extravagant; but you will think otherwise if you reflect for a moment on the exact and perfectly defined character of the terms employed in other sciences. Thus, in physics, the instrument that measures the weight of the air is not called an hygrometer, but a barometer; and the same precision exists throughout that and all other sciences. Why should medicine, then, be alone without positive terms and technical phrases of precise and determinate signification? Again: What on earth is the meaning of the word *irritation*? An obstacle of some sort or other modifies the course of the blood in any given organ, and instead of simply stating that there is a modification produced by a mechanical impediment to the progress of that fluid, you tell us the part is *irritated*, and actually employ the very term the worst fitted to designate the disorder manifested in the circulation. How long, I should be glad to know, have our organs been proved susceptible of feeling passions—of becoming irritated—I had almost said of getting angry? The medical language of our schools is almost from one end to the other an incoherent metaphor. Figures and tropes have their merit, I allow; but their fit place is in a poem, or a romance, and the science we cultivate will, I trust, cease to be ranked with such performances.

I cannot be surprised that the study of the properties of the blood has been neglected, for such neglect is in harmony with the habitual conduct of medical men. Instead of seeking, by careful experiments, the elucidation of important problems, they waste their time and energy in futile disputes. The verbiage and subtlety of the bar seem to have spread to the members of our profession. Whoever talks loudest and longest usually triumphs, and, of course, settles the matter his own way: he does so often in defiance of truth; but what matters for that? he gets himself spoken of and makes a name. On the other hand, the man who does not possess that extraordinary gift of the tongue, by which white is made black and black white, remains unknown, vegetates, and passes for an ignoramus in the eyes of the public. I really cannot help occasionally falling into these digressions, I feel so strongly the degraded state of the science and the retrograde course that people strive to make it run. I am persuaded that I am doing the student a real service in warning him against the crowd of errors that are obstinately promulgated by medical writers and professors.

But to return. We ascertained that the first condition for the accomplishment of the circulation, was that the blood should have the property of forming into a mass when removed from its vessels and left to itself. This is the fundamental fact to which the majority of those we shall subsequently become acquainted with, will be found referrible. Meanwhile we will endeavour to discover the physiological or chemical circumstances, and the particular substances that influence that phenomenon. There are some that diminish, others that immeasurably increase its activity. Among the former we have already become acquainted with a certain number, such as the alkalies, employed, as I mentioned in my last lecture, for the saturation of uric acid in the kidneys and bladder. The gases we breathe, our food, &c., modify that property also; neither must we forget asphyxia by lightening and by carbonic acid, nor the action of hydrosulphate of ammonia, all of which liquefy the blood with more or less rapidity. Suffer me to remind you, too, of the details of an experiment I performed with œnanthic æther. That liquid, as its name indicates, (οινος wine, and ανθος a flower) is considered by its discoverers, MM. Liebig and Pelouze, to be the principle that gives wine its peculiar aroma, or *bouquet*. In other words, the quality of one wine is superior to that of another, because it contains a larger proportion of œnanthic æther. I leave to the palates of the connoisseurs the experimental solution of one part of the question; but as to the physiological relations of the fluid, here is what I observed:—I caused a drachm of it, mixed with an equal quantity of distilled water, to be injected into the veins of a dog. The animal immediately fell down, with every symptom of drunkenness; but it was not alone in feeling its effects; the assistant who prepared the æthereal solution staggered as he brought it in; and the manner in which he introduced the injection

confirmed my idea that the liquid took effect in the form of vapour. The dog remained motionless, became comatose, his respiration grew loud and stertorous, and he died in three-quarters of an hour. The post-mortem examination was made the following day:—On incising the muscles of the trunk, a quantity of fluid blood poured forth from the vessels; it seemed as if no clots had been formed in their interior. The lungs contracted on opening the chest, as in the normal state, nevertheless their physical properties were much altered; they presented the greater part of the signs of engorgement; among others, augmented weight and density; but there was no true hepatisation—a new proof that that particular condition of the pulmonary parenchyma is connected with the coagulating property the blood possesses while living. The heart and great vessels were filled with blood, which was more viscid than that of animals killed by the carbonate of soda, though its appearance was the same to the eye. The internal surface of the vessels was studded with brownish patches, produced by the imbibition of some of the elements and of the colouring matter of the blood: no fibrinous clots were to be found. The liver and spleen were evidently enlarged; when cut into, a quantity of black viscous blood followed, in greater abundance than usual. The condition of the kidneys and other parenchymatous viscera was the same. The external surface of the stomach and intestines was streaked with reddish lines, radiating in various directions, and forming a rete by their interlacement; these were the capillary vessels distended with fluid blood. There were no extensive patches of extravasation, for death was so rapid that the liquids had no time to penetrate the walls of the intestines by imbibition. Now, let us suppose the body of an individual opened at one of our hospitals, who had been suddenly carried off by some disease of unknown nature (and the catalogue of such affections is a tolerably long one); and let us suppose lesions similar to those described to be discovered, would the observer hesitate for a moment to attribute them to inflammation? Certainly not; he would pronounce the case a beautiful example of gastro-enteritis. The most incredulous bystander would be forced to yield to the conclusive evidence afforded by the condition of the mucous membrane. Who could have the face to utter a syllable of dissent when he saw that membrane swollen, red, arborised, and covered with a layer of viscid sanguinolent matter? Be that as it will, however, I found no local lesion in the subject of my experiment capable of explaining the severity of the symptoms and rapidity of death. The whole economy was affected; non-coagulable blood circulated through the system, and, as the walls of the vessels were unable to prevent its imbibition, it deposited its materials in every direction. The first cause of any organic disorder that might have ensued was evidently to be found in the physical modification, the liquefaction of the blood. I have no doubt but that by following up this by similar experiments, we should come at some facts calculated to throw light on the physiological effects of wine. When habitual drunkards fall ill, the phenomena their complaints present are of a special character, and require peculiar treatment. As the sudden introduction of a little œnanthic æther into the veins of an animal destroys the coagulability of its blood, it is by no means impossible but that the prolonged abuse of wine may, in the end, entail similar modifications in the physical properties of that fluid. Much has been written on drunkenness, its effects, and the disorders it induces in the organic functions. The pathological anatomist has examined every organ in its turn in search of the peccant principle, but in vain; in spite of all that has been done, conjectures are all that we have got. Delirium tremens has been attributed to inflammation of the brain, of the cerebellum, of the meninges; but not a word has been said of the condition of the fluids. It will be my aim to investigate these different points thoroughly; they are of the last importance, for it is by examining the condition of the blood that we have learned the mechanism of the production of several of those terrible diseases that decimate the human species. Thus we have been enabled to explain the black vomiting of the yellow fever, that devastates the shores of America. In the first place it is known that the deleterious miasmata, rising from animal or vegetable matters in putrefaction, produce that terrible disorder; but its immediate cause, the middle change between the action of the miasmata and the appearance of the disease, had been unknown. We have now ascertained that that change is none other than the liquefaction of the blood, which, in consequence of its altered state, is effused into the abdominal viscera, disorganises them, causes their sphacelation, and gives rise to the pathognomonic dejections of that frightful disease.

Continuing our scrutiny into the properties of the blood, we soon discover another of its special characters. This is a peculiar viscousness. Now, on first thought, this would seem an obstacle to its passage through the ultimate tubes, but it is, in reality, an indispensable condition for its free circulation; to such a degree, indeed, is this true, that viscousness and normal state of the blood are two inseparable ideas. This property, again, is illustrated by experiments, made on inorganic tubes. If we endeavour to introduce water into a tube of extremely small diameter, that liquid, as I have already mentioned, will not enter it.

no matter what force be employed; but if a certain quantity of any mucilaginous substance, such as gum, gelatine, or albumen, be added to it, the attempt at injection becomes successful immediately. This fact has been established by some ingenious researches of M. Poiseuille, the results of which are not yet published. It affords a fresh illustration of the utility of the study of physics in their application to physiology; it shows, too, what little regard should be paid to those arrogant inventors of vital laws, who not only themselves disdain the light shed by the phenomena of physics on physiological actions, but do all they can to prevent the rest of the world from availing themselves of it also.

In spite of whatever vitalists may, in their pompous style, urge to the contrary, I look upon it as incontestable, that the blood circulating in our organs, may be justly assimilated to the fluid in the experiment adverted to. If it lose its viscosity, its further passage becomes quite as impossible as that of the non-mucilaginous water; it stops at the entry of the capillary system, is extravasated into the surrounding tissues, and causes the disorders which those sage personages ascribe to irritation and inflammation. The prevalent mode of treating disease harmonises admirably with, and is quite as senseless as, this method of reasoning in pathology. The practitioner mixes, combines, and jumbles together vegetable, mineral, and animal substances; administers them right or wrong, without considering for a moment the cause of the disease, and without a single clear idea on the why and wherefore of his conduct. You may prove to him, as you will, that this or that substance is insignificant, useless, or even hurtful; little will he regard your expostulation. And why should he, when, by readiness in prescribing a monstrous farrago of drugs, he knows he shall acquire the reputation of being *profoundly versed in the materia medica, of being a man of immense resources, &c.*? Such, Gentlemen, has ever been empiricism.

We were speaking of the viscidity requisite for the circulation of the blood through our organs. Now, here is the blood of an individual who had an attack of hemoptysis, and was bled freely for it. You know well what I think of that remedy,—worse, perhaps, than the disease. Be that as it will, you may perceive that this blood is very slightly viscous; I, in consequence, presume that further mischief will occur. We shall see if my presumptions are realised. We have no instruments to measure the viscidity of fluids; we are, therefore, instead of having an accurate and rigorous estimate of that of the blood, reduced to simple conjectures. I consider that the discovery of any method of measuring it with exactness would be a most valuable acquisition. Meanwhile, we will do what we can to determine it with the areometer. There is further proof that a certain share of viscidity is an essential requisite for the healthy circulation of the blood. If you bleed an animal several times, and replace the blood withdrawn with water, exhalation and effusion into the cavity of the pleura will follow, and, subsequently, into the peritoneum. Now, you have done no more than diminish the viscidity of the blood, by adding a little water to it. But, on the other hand, if you try the converse experiment, and augment its viscidity beyond its natural term, the circulation ceases altogether, in consequence of the affinity between the molecules of the blood being rendered too great. These molecules adhere to the walls of the vessels, and impede the circulation, just as blocks of ice, stuck to the banks of canals, or rivers, interrupt the course of their streams. Hence there must be diseases originating in excessive viscousness of the blood.

You remember the experiments we made to exemplify this question. We added a viscid matter, innocuous in itself,—gum, for example,—to some water, and, after colouring the fluid, injected it into the jugular vein of an animal. So long as the injection traversed the large venous trunks no disorder was occasioned; but once it had arrived through the pulmonary artery, amongst the minute ramusculi of the lung, its degree of viscidity ceased to be in just proportion to the capacity of the tubes. The consequence was, that the circulation almost instantly stopped, and, as the encephalon no longer received the necessary excitement of arterial blood, its functions ceased, and the animal quickly perished. The autopsy was immediately made, and, on incising the pulmonary parenchyma perpendicularly to the direction of its principal vessels, we invariably found them stuffed with the substance injected. Let us admit, however, that this liquid had succeeded in making its way through the capillary system of the lung. You are aware that the diameter of the ultimate tubes varies almost in every organ, and that in some of them it is still smaller than in the lungs. Let us suppose, then, any substance that has passed through the pulmonary parenchyma with great difficulty, arriving at other capillaries of greater tenuity. It will, beyond question, be arrested in its course by this new obstacle; and its stagnation and subsequent effusion will produce, according to the nature of the parts with which it is in contact, various disorders, more or less analogous to those already described. In both cases the cause of the mischief will be the same in principle, namely, the obstacle the circulation encounters from the want of harmony between the globules of the liquid and the tubes to be traversed. It is in a case like this that a precise and even minute knowledge of anatomy

would be useful in physiology; but as the former of those sciences is very superficially studied, we are invariably found wanting when the application of anything like profound acquaintance with it is required.

I mentioned gum as a substance that increases the viscidity of the blood; I may add that oil and starch, and all amylaceous matters generally possess the same property. Besides, similar modifications arise spontaneously in certain diseases. Thus, we sometimes meet with blood so extremely viscid, that it has very nearly the same consistence as, to use the language of pathologists, currant jelly. I showed you several cases of this kind, both natural and artificial; and you saw that the most perfect analogy always existed between the phenomena produced by the unknown causes of disease, and by my experiments. In every case in which you find the blood clotted in this manner, you may rest satisfied that the lungs have been the seat of some profound lesion. We have ascertained that the alkalies liquefy the blood; and it is similarly demonstrated that certain acids, sulphuric among others, increase its viscidity, by combining with, and solidifying, its fibrine. Nay more, I have proved, as in every other case, by experiment, that prolonged inanition produces the same result; the blood loses its aqueous principle, and tends to solidification. We will repeat the experiments from which these important conclusions were derived. Experiments should, indeed, in all cases be repeated with care, for their execution is not such an easy matter as might be supposed, nor is all the world capable of performing them properly. Besides, we shall, perhaps, by their repetition, discover some new facts, which escaped us in their first performance. The density of the blood must, also, be taken into consideration in the theory of the circulation. This property has, it is true, been noted by observers; but no attempt has been made to investigate the results of the different modifications to which it is liable. We know that its density is somewhat greater than that of water; still this simple fact is of little importance; it tells us nothing positive on the degree of density necessary for the circulation. Here is another property—it were to be wished we could find out some accurate method of measuring.

But there are important facts, of another order totally, to be learned respecting the blood. When we examine that fluid, while yet circulating in the living animal, we are immediately struck with its heterogeneous appearance. It is seen to hold myriads of little particles in suspension, rolling on each other, and intermingling in a multitude of ways. These particles, called globules, are known to have determinate dimensions, and to affect particular forms, according to the class of animals in which they are observed. They are elliptical in fishes, reptiles, and birds; in the mammalia they have the appearance of a circular lens. These globules deserve our close attention, for when they undergo certain changes they are unfitted for circulation in their tubes. However, it must be remarked, that, of all the elements of the blood, they undergo the fewest modifications in disease. As I before mentioned to you, we must be careful not to draw conclusions from one class of animals to another; to suppose, for example, that these bodies have characters identically the same in fishes, birds, and mammalia. Their real characters are, on the contrary, materially different. In the first-named class of vertebrata the globules are of two kinds, small and large, and have all of them a central nucleus; when washed and shaken in water, their outer investment is dissolved, but the nucleus remains. This is not what is observed in mammalia and birds. In order to obtain the globules in these classes, it is not enough to submit the clot to pressure; we must beat up the blood as it flows into the receiving vessel, just as we should if we wished to separate its fibrine. By these means the globules are precipitated, and by pouring off the supernatant liquid we obtain them in a perfectly isolated state. If examined under the microscope in this condition, they, too, are seen to consist of a sort of investment with a nucleus in the centre. But both investment and nucleus disappear by washing, and the water employed becomes red-coloured, like the serum, in some diseases. From this I conclude, that in mammiferous animals these corpuscula are not formed of an investing substance and nucleus analogous to those of fishes. Sanguineous globules are thus divisible into two classes; those with and those without a nucleus. The former belong to reptiles and fishes; the latter to mammalia and birds. The globules of other animals have not been sufficiently examined in this point of view, to admit of any opinion being formed respecting them. The doctrine I have just stated is, I am aware, at variance with that maintained by some physiologists. I give it to you, however, as the result of the experiments I have made on the subject; I will cease to hold it when it is proved fallacious. It has also been advanced, that the globules are composed of a sort of proper parenchyma. According to this view their superficies are formed of *hematosine*; the parenchyma itself of albumen; the central nucleus of fibrine; but the perfect solubility in water of the globules of mammalia and birds, makes me suspect the correctness of this account.

The point to which I am anxious, at present, more especially to call your attention, is the dimensions of these corpuscula. It has been asserted that their size varies con-

siderably in the human subject, and that, under certain circumstances, their diameter does not exceed the 100th part, the 120th, or even of the 150th part of a millemetre. Their magnitude does vary; but I feel almost certain that their diameter never exceeds an eightieth of a millemetre. Now, this point of limitation to the size of the globules is extremely remarkable from the relation it bears to the diameter of the vessels in which they are destined to move. There is in this an admirable harmony between their volume and the capacity of the capillary vessels; so accurate is it, that I am persuaded, if we injected the blood of a fish, or reptile, into the veins of a mammiferous animal, various disorders in the organs would quickly supervene, from the simple reason, that the size of the globules of the former would be disproportioned to that of the vascular tubes of the latter. I shall have recourse to direct experiment for the confirmation, or the contrary, of this hypothesis. I will procure the blood of a sufficient number of frogs, and inject it into the veins of a guinea-pig, or some other of the mammalia. Meanwhile, there are two facts that give considerable weight to the opinion I have started. They are these,—when we add, to the blood, globules of the feculæ of corn, or potatoes, which are perfectly innocent in their properties, but vary from 1-20th to 1-10th of a millemetre in diameter, we invariably produce an obstruction of the capillary vessels, followed by the consequences with which you are well acquainted. If, on the other hand, we substitute for these globules others similar in nature, but of infinitely smaller size, the circulation continues uninterruptedly. Thus, I injected some of the fecula of the mirabilis jalapa, the granules of which are only 1-300th of a millemetre in diameter, into the veins of a dog; and though the experiment was repeated twice in the same animal, no notable disorders followed. Subsequently a larger quantity of the liquid, containing the fecula, was injected by another person, but as it had meanwhile lost almost all its water by evaporation, the solution had become very thick. The consequence was, that the animal was seized with intense dyspnœa, and died in a few hours; but its death was to be ascribed to the viscidity of the fluid, and not to the size of its granules. The dimensions of those little bodies were the same in the solution that caused death, as in that which had twice traversed the lungs without injuring the animal. But a stronger proof still of the justness of accounting thus for its death is, that the appearance of the lungs was precisely that observed in animals who die from an injection of oil, or any other viscous fluid. Hence, to recapitulate, in a few words, what I have been saying, if we view the globules as corpuscula suspended in the serum, it is fair to conclude that they might, without causing inconvenience, be smaller than they are; but that if their size were increased beyond a certain point, it would constitute an insurmountable obstacle to their passage through the terminal vascular tubes. Their mean ordinary diameter varies from the eightieth to the hundred and twentieth part of a millemetre. There are some considerably smaller, but, in all probability, they are of a different species from those of which we have been speaking; to ascertain this for certain, however, would be a very interesting as well as novel topic for inquiry.

If we turn from these purely physiological questions to the chemical history of the blood, we shall find in it many points quite as interesting as those we have rapidly reviewed. The difficulties encountered in the prosecution of that branch of inquiry are not less serious than those met with in investigating its physical properties. However, it is easy to establish one important fact, namely, that every anormal chemical modification of the blood is followed by morbid phenomena, of just as great gravity as those which have already afforded us subject for discussion.

I may here announce that my old pupil, M. Denis, of Commercy, has made some very curious researches on the chemical composition of the blood; among other important facts, it would appear to result from his labours, that the fibrine is nothing more than albumen combined with different salts. For the present I neither adopt nor reject this opinion,—I wait for the proofs of its correctness. M. Denis has presented a memoir on the subject to the Academy of Sciences. He proposes repeating his experiments in public at the Ecole Pratique, and I strenuously recommend you to attend his lectures.

Among other practical points of importance to which these general views have led us, we have ascertained that venesection modifies the relative proportion of the serum and clot. Here is the produce of the fourth and fifth bleedings, which were practised on the animal I showed you the other day. In the fourth the serum is to the clot as 55:45; in the fifth as 65:35. Now this is surely a very great difference; yet in this case the various abstractions of blood took place at intervals of two days, and the animal was fed well all the while, which prevents any very rapid alteration of the blood; and not only is the clot modified, but the serum also becomes whitish, and is pretty often found covered with a layer of opaline matter.

In fine, the nature of the blood, and of its different elements, is an important question in a therapeutical point of view. We can now maintain, with confidence, that it is not a matter of indifference whether we bleed little or much; whether we draw a small or

a large quantity of blood in a very short space of time, or at distant intervals; whether we push the abstraction to syncope, as has been advised by many writers, or repeat it to a smaller extent, at several different times, with a variable interval of time between them, as learned practitioners of the present day recommend. We are also justified in proclaiming, that men who bleed without giving themselves the least uneasiness about the disorders that follow the removal of blood, both in that fluid itself, and as their consequence, in the various organs; who look on these disorders as curable by bloodletting, while they are, in reality, produced by it, act with most reprehensible blindness. In simple language they do mischief, where they imagine they are doing good; and in many an instance, on their doing that mischief or that good, the death or recovery of their patient depends.

ST. BARTHOLOMEW'S HOSPITAL.

INTRODUCTORY CLINICAL LECTURE.
SKETCH OF MR. STANLEY.
THE SYSTEM OF "REGULAR COURSES."

It is inappropriate to inquire here how Mr. Stanley obtained office in St. Bartholomew's Hospital; but at least *this* is certain, that for his post he was not indebted to the unmeritorious influence of personal attractions. The remark is not invidious; the sketch of any lecturer would be incomplete without a notice of his bodily as well as his intellectual characteristics. Some public men are great in the one respect only; some in the other; some in both. The announcement of either fact is essential to a likeness, and, as a fact, we simply state, that Mr. Stanley is a lecturer of humble appearance and unpretending manners. A dazzling front might deceive an observer into a wrong estimate of his merits.

He lectures sitting. That position reduces what a vain man might consider to be his disadvantages, half the person being screened by the table, and his height disguised. He thus addressed a full audience, in the medical theatre, on Saturday, Oct. 6, delivering on that occasion an "introductory clinical lecture," the first for the new session. If equal punctuality and zeal distinguish him on every Saturday, at twelve, before its close, he will deserve a prize, which the students should institute for the most regular and communicative of their medical teachers in the hospital. To hear Mr. Stanley expatiate on the study of medicine, would persuade any pupil that he possessed in him a most faithful teacher. He appears to regard clinical instruction as the most useful department of medical tuition. He was earnest in his remarks, warm in his praises of "practical knowledge;" few lecturers, from manuscript, could impart a greater appearance of sincerity to their expressions on its behalf.

When a very youthful student hears high praises of clinical instruction from an influential teacher, he may naturally ask why certificates of his attendance on every kind of medical lecture but clinical are demanded by the Examiners at the Colleges. Each precept of the "regular courses" can be read in books. Clinical lectures can be obtained only at the bed-side. The demand, in either case, is unwise and unjust, because the examination, and not the certificate, is the test of knowledge. The oral proofs of candidates are certain, the written proofs of lecturers are fallacious. The latter indicate only the payment of money, or may be forged; the former are unerring testimonies of knowledge or ignorance. How comes it, however, that since the certificate system does prevail, certificates of attendance on clinical lectures are not demanded? And how is it, more strange still, that these discourses are so highly vaunted in the introductory lectures at the hospitals, while the others are but coolly noticed? The former are the baits of the hospitals. The pupil hears them praised; he anticipates from them a fund of instruction; he knows that hospital practice is valueless without them. He is caught by the anticipations which they excite, having wandered from hospital to hospital to learn the kind and amount of knowledge that he may expect in either. He guesses correctly that the mere permission to *look* at the patients is useless,—is a poor return for the enormous fee. He derives, at the introductory lecture, a belief that to witnessing is added the essential auxiliary understanding, and, according to the assurances which enforce that belief, he pays his money at the hospital door. To cheat him afterwards may be shameful, but that is another thing. But why are not those important adjuncts of "admission to hospital practice," clinical lectures, on the College regulations? Alas! an essential to their being *attended* is that they are *delivered*. However useless the "regular course" lectures,—those stale compilations on the practice of physic, therapeutics, midwifery, anatomy, botany, jurisprudence,—at least they are read to the student. Common decency compels compliance with that form. The lecturer takes the money for, and gives a ticket of admission to, his course. He is party to an express agreement, which cannot be neglected. Suppose the students were to go in a body to the College, or Hall, and say, "Gentlemen Examiners, here are our certificates, all signed, but we have had no instruction. Not a lecture has been delivered this winter. They took our coin, but were too lazy, or too haughty, to give us the lectures." The astonishment and the result may be readily guessed; but suppose the same body, on presenting their hospital

credentials, should say, "We have daily walked the wards, have seen all the patients, have overlooked all the operations, but we have had no clinical instruction, not a single lecture from any one of the medical officers." "Well, Gentlemen," would the President reply, "we regret the omission, but we must receive your testimonials. You will soon learn the practice of medicine from experience. Experiments and post-mortems will teach you all. Here are your diplomas. Pay Mr. Balfour."

Yes, the hospital fees are obtained without stipulation to teach. To be "free of a hospital" is the one thing needful at the College and Hall. To receive lessons in medicine there is another thing. Students are compelled to take clinical instruction in them for granted. No wonder that the delusion is sustained so extensively. He readily supposes that tuition is part of the bargain he is about to make; and so it is. But what penalty is there for the breach of faith? The student can break no promise. He pays beforehand. He is obliged to do one of those "three things which make a bad paymaster."

But will not the expectations of the students induce the medical officers to fulfil this duty, though it be not in the bond? Sometimes they do, but rarely! The clinical tuition is an extra trouble, and can be escaped. The promise in the prospectus is issued anonymously. The writer stands behind a screen, and cannot be confronted when deception comes out. Probably a hospital prospectus, with "Clinical Lectures by all the Surgeons and Physicians," issued in October, could not be found in all January. We never ourselves saw such a thing in that advanced month. Perhaps the Secretary buys them up for shame. Knowing all,—looking back on the last ten years,—it is quite laughable to hear the solemn injunctions of the "introductory" lecturers at the hospitals to the students, to profit everything possible by the vast field of instruction that lies before them. One seems bound to believe such sincerity of manner to be real. The students are even reproached, by anticipation, for being less eager to receive than the teachers to give. They cheer the lecturer; they intend to mend; they will not miss a case, nor any one of the six lectures a week which the six medical officers will give. They are anxious to begin; they fly to the wards. How often have they ended the session in bitter disappointment! For what have they been dragged to London?

Let us hope for better things even this winter. The press has already worked a great reform in this matter, and there can be no retrograde movement. The students themselves begin to see their true position in the hospitals, and to know their moral rights; and at least in the early part of the session, complaints may not have cause to be made; the duties of tuition will, in many instances, be cheerfully and honourably performed; but if the students themselves relax in *expectation*, so will their teachers in fulfilment; punctuality will first yield; then will follow occasional neglect; then total; but *not one* lecture should be foregone. We try to excite no exaggerated fears. Not two years since Mr. Morgan made, at Guy's Hospital, as fine promises in an "introductory" lecture as Mr. Stanley,—finer, indeed, for he promised more for himself and less for his colleagues; yet the surgeon at Guy's gave but two clinical lectures in six months! Repeated remonstrances were made of the neglect, in this Journal, by the pupils. To these, at last, a friend on behalf of Mr. Morgan replied, that Mr. M. had qualified his promise, in the introductory lecture, with the word "occasional," and that he therefore regarded the complainants as very saucy fellows, whose querulous impertinencies would prevent him from delivering another clinical lecture that season. All this time the surgeons of the hospital took very good care to read their lectures on Anatomy, Surgery, &c., productions which, in general, call into requisition very different qualifications from those which are demanded in an extemporaneous lecture on cases in the hospital. The writers of the former resort to cyclopædias, dictionaries, First Principles, Studies of Medicine, for ready-prepared facts. With few exceptions they draw but little on their own experience. They copy out, a month or two before their maiden session, the paragraphs which serve to make up a course of sixty lectures. This "course," with now and then additions from the journals, as medicine advances by other aid, does duty, perhaps, for twenty sessions. It is read and re-read between the October and April of every year; for instance, the lectures on surgery now delivering at St. Bartholomew's Hospital, appeared in print six years ago; and last year we found six old copies of the introductory lecture under the scrutiny of as many students, in the theatre, who were following the lecturer, word for word, as he repeated that discourse by heart, with fluency so excellent and dramatic, that, unaware of the fact, no hearer could have guessed it. The cost of the separate sheets on which the entire course was printed might be rated at 6*s.* 8*d.*, and the possessor might read their contents "perpetually" for that sum, without coming to London. The charge of Mr. Lawrence for a ticket of "perpetual" admission to his antiquated course, is, most modestly, 8*l.* 8*s.* But the clinical lecturer must exercise some other powers than those of industry, transcription, and the lectorial faculty; therefore the lazy man avoids the task. He must have no "authority" but the case-book.

To save himself from disrepute, he must be able to draw on accurate observation, extensive personal experience, and sound judgment, without fear of intellectual bankruptcy. The rulers in the corporations place themselves in no such predicament as this.

The quality of eloquence is not essential in a clinical lecture, for Mr. Stanley accomplishes the intention of a teacher without possessing a single supernumerary elocutionary power. He is earnest, and plain, and simple statements require no graces of oratory. His eloquence reached the extremest verge of its narrow limits in his preliminary eulogium on "the school of St. Bartholomew." Questionable is the taste and propriety of such an advertisement. Think of Dupuytren puffing the Hotel Dieu; or Louis, La Pitie! But both interest and vanity operate under the existing system of medical government in the English Hospitals. No feeling is more strong in the London hospitals than the opinion of each staff that its own establishment, officers and subs, patients and medical officers, is the best in the metropolis.

Probably this opinion extends even to Charing Cross. Certainly it reigns in all the larger buildings. Thus, in the Borough, Mr. Cooper maintains the superiority of Guy's from the nurses and steward up to the unmatchable treasurer. In Smithfield, Mr. Stanley commenced by recommending six prosy lectures of "one of our physicians" as the very best guide for practical study in London, "where there is no time to be lost." In those lectures "you may learn how best to avail yourselves of the events of hospital practice. You have not time to watch daily the progress of patients." —No, the clever and disinterested arrangements of the medical corporations prevent that. For one whole year "your observation of cases must be desultory, and made *between the intervals* of lectures." They must be clever fellows who seize those intervals. A witness in a court of law announced an event to have occurred "between the hours of 3 and 4." Counsel immediataly urged that plaintiff must be nonsuited, for directly that three o'clock ended, four o'clock began. The "intervals" of lecture are similarly appreciable. Mr. Stanley held up hospital practice like the apple of Tantalus. "In the wards of St. Bartholomew's most valuable knowledge may be gained. There is so much practical work going on in this great establishment, that something of practical utility presents itself at every moment of the day. The multitude of its in and the multitude of its out-patients ensure this result." It is to be hoped that some more wholesome regulations will ere long release the student from the profitless drudgery of compliance with the time-wasting ordinances of the certificate system, and permit him at once to realise the only object and advantages which can excuse his imperative transportation from a competent provincial school, to the temptations of a vicious metropolis, whose dissolute vortex it is not easy to avoid in an almost venial endeavour to escape from a bewildering, wearisome, and disgusting routine of sham studies.

On entering the theatre the students offer plaudits, which Mr. Stanley notices by some curt nods, directed abstractedly, as though he might be saying within, "Ah! Don't trouble. There you are. I hear you. He generally opens his notes at once, and without palaver begins with the usual "Gentlemen." As a lecturer, he gains nothing by a release of his eyes from the manuscript which may lie before him. Nor does he lose by the change. He did not trust the first part of his lecture to his memory or his wits. It was prepared to hand. He fortified himself to begin, by a readjustment of his specimens and casts, and a peep into his scalpel case, as another professor might settle his wig, or strengthen his nerves with a pinch of rappee. The lower jaw would threaten thick speech, but a clear enunciation disproves the diagnosis. Mr. Stanley's discourse may be followed with ease. His advice to the students was practical, but not novel. It was confined chiefly to one recommendation,—the frequent use of the pen. "Begin your studies, Gentlemen, with Locke's Common-Place Book, putting down every fact which you may hear or observe, in the order of its occurrence One of the most accomplished members of our profession told me that this was his mode of acquiring knowledge. He entered strictly in it everything that fell under his notice. It was true that, in after years, on looking over his memoranda, he found much that was almost valueless, but the habit had disciplined his mind. Dr. Baillie strongly recommended short notes on every occasion. To think, to arrange, and to write, were, he said, the great duties of the student. Cases, observations, insulated facts in medicine, all should be noted with the pen. In addition to the matter thus accumulated, the memory itself would be greatly improved. Inculcate, then, unceasingly, these two habits, memory and writing. The celebrated classic, Porson, possessed two remarkable peculiarities, an extensive memory, and a love of the manual practice of writing. He has stated that he never remembered anything which he had not recorded three times, and read six. In this way medical knowledge is to be obtained. As to those students whose first winter in London has passed, they should daily and diligently observe cases. Nothing should escape their attention in the hospital, employing every means in which energy and perseverance can be used, to meet the responsibilities which will by and by fall upon them in the practice of surgery."

These quotations, slightly condensed, show the style of a lecturer who, if he be not striking and impressive for his matter, must become insignificant in the professional chair. He cannot make much out of little. He does not attempt it. When his thoughts as well as his perceptive faculties were employed, in the change from manuscript to extemporaneous speaking, the ear could perceive the fact, but was not long annoyed by hesitations, for soon as he abandoned generalities, and began to deal with clinical *facts*, he was sufficiently fluent for the purposes of instruction. To be eloquent on disease, as we have already remarked, is not essential. Clinical medicine wants not the rouge of oratory. Having premised that in these lectures he "did not propose to give formal, finished, discourses, though he called them clinical,"—that he should "only relate what occurred under his own observation in his own cases, with a view to exciting attention to them,—presenting, in fact, a *catalogue raisonnée* of his patients in the hospital. He should first of all direct attention to those cases in the hospital which were remarkable for rarity, or practical points; then to their treatment; then say why and wherefore certain things were done or left undone in the treatment." He added that "it would be the students' own fault if they did not profit by the opportunities that were before them. The names of the diseases hung above the beds; what the pupils did not know they might learn on inquiry; his colleagues, as well as himself, were most anxious and ready at all times to answer questions, therefore the students should take care not to let cases pass without fully comprehending them;"—after making these announcements with sober feeling, he exemplified his clinical plan by drawing attention to three cases which had been admitted under his care:—

Division of the Peroneal Artery.—"The first case, Gentlemen, is that of a man, wounded in the leg, a butcher, who, on Monday last, (Oct. 1) while slaying an animal, slipped his knife, and penetrated his leg, two thirds down on the tibial side. The hæmorrhage was profuse. You could track his steps through the archway of the hospital by the blood. The house-surgeon immediately compressed and bandaged the limb, leaving it for a short time to consider what should be done, and sending a note to me to attend. It was plain that one of three arteries was wounded; the question was, which. It could not be the posterior tibial, for that was felt beating. Was it the anterior tibial? No, for that also was beating. Then it was the peroneal, the artery which is placed behind the fibula. Must this vessel then immediately be tied? Those who know its situation will see the difficulties of that operation. The flexor longus pollicis overlaps it. Hey recommended us to excise a portion of the fibula to get at it. This shows the seriousness of the proceeding. But Mr. Guthrie proved that there exists no danger in dividing the large muscles. To reach the peroneal artery he divided the calf,—cut through the gastrocnemius and soleus muscles. Previous to that case, surgeons were apprehensive that such a division was not practicable with impunity. Now, if the present accident had been a wound of either the posterior or the anterior tibial arteries, I would immediately have employed a ligature, but the difficulties of reaching the peroneal artery induced Mr. Lawrence to concur with me that the attempt was not justifiable, unless the hæmorrhage imperatively demanded it, or aneurysm subsequently ensued. So the wound was left to nature, and at present all proceeds quietly, with a chance of avoiding an operation. You will by and by learn whether this is right, or whether serious hæmorrhage, or aneurysm, ultimately occurs. Aneurysm does repeatedly happen in similar wounds of the hand, and it may in this case. Suppose that I am at last obliged to tie the artery. I cannot cut into the existing wound to reach it. That must rarely be done. A fresh incision must be made on the outer and back part of the leg, down to the artery which circumstances induce me to consider is wounded.

[*Saturday, Oct. 12th.* To-day the lecturer again drew attention to this case, in the following terms:—]

"Gentlemen:—The question whether there would be a closing of the artery, or a return of hæmorrhage, or an aneurysm, was answered on Tuesday night last by profuse hæmorrhage, and the ligature of the vessel became imperative, though I did not tie it until the next day; the difficulty of doing so at night made me wait. I had a tourniquet and bandages applied, and left him till the morrow; and I say to you, *never* attempt to search for a posterior tibial, or a peroneal, or any other difficult artery, in the absence of daylight. The man did not do very well after the operation. Inflammation of the absorbents came on; red lines appeared on the inside of the leg up to the thigh. I ordered him tartar emetic and salines; and fomentations and poultices to the part; but Mr. Williams finds that the man has been a hard drinker, and there is reason to fear that we shall have to combat in him delirium tremens."

(Mr. Stanley afterwards ordered the man, in the ward, four tablespoonfuls of hot brandy and water, in equal parts, immediately, to be repeated every hour; to have porter also, and laudanum at night.)

"*Bursal Tumours of the Wrist.*—Here is a cast of the forearm and hand of a woman with a tumour on the palmar aspect, divided into two portions, with a kind of hour-glass contraction, by enlargement of a bursa under the annular ligament, which ligament,

pressing on the distending tumour, divides it into a smaller swelling immediately above the hand and a larger on the other side of the hand. What are its contents? Possibly small solid bodies, like half-boiled sago or rice. What was the treatment? On Saturday last I punctured it with a needle, to see if only transparent fluid would issue. None appeared, so I enlarged the aperture, when all the little tumours in this bottle came out. They are a fair sample of the contents of such tumours. Why did I first use a needle? Because we should be careful of wounding a bursa,—a bursa of the olecranon, for instance. As little injury as possible should be inflicted on bursæ. No constitutional derangement followed the evacuation, but the fluid has reaccumulated, for which a compress was put on in the hospital.

"*A new Mode of Managing Simple Fractures* induces me to mention this subject. It consists in the simple application of splints after the swelling and inflammation have subsided, to prevent movement of the ends of the bone. We owe this improvement in surgery to Mr. J. Lawrence, late our house-surgeon, and it is now much used on the continent, especially at Brussells. See the splint made, Gentlemen, and learn to apply it yourselves. The patients, four or five days after the occurrence of the injury, can move about with a sling and crutches. Nay, in a week or ten days they walk about, bearing considerable weight on the limb. There is a woman in the hospital, who, in ten days after a fracture of the tibia, walked with comfort, with a crutch. This view of the subject cannot but strike you with pleasure, and the old surgeons, could they now witness the change, would be astonished.

Enlarged Scrotum.—"Yesterday you saw me taken up suddenly into St. John's Ward by Dr. Roupell. This was to see a man, in an advanced stage of fever, with the scrotum much enlarged, dark-coloured, and threatening gangrene. I detected emphysema immediately. The question was, did air exist in the scrotum, or was there an effusion of urine into it, or what caused the swelling. The man said that he had had a difficulty of making water. This fact was important, for if the contents were blood, two or three punctures, merely, would suffice; but if it were urine, the incision must go far deeper, leaving the swelling of the scrotum itself out of the question. A division through the perineum into the bladder must be made, to evacuate the fluid, and prevent farther effusion of urine. I made two free incisions into the scrotum, fomented the part, and desired that a catheter should be introduced; but the man died in the afternoon, and, I regret to say, the body, as is too often the case, was removed by friends, so that we shall not be able to examine it,—at least not in the hospital.*

"So much, Gentlemen, for cases in the hospital. When operations are to be performed, we will take care to tell you the nature of the cases. To day there will be two; one for club-foot, by section of the tendo-Achillis; the other for stone. I am going to perform lithotomy, not lithotrity. Why? Surgeons are bound to consider what cases are best suited for either. Lithotrity will not displace lithotomy, but accompany it.

"*Melanosis.*—I have before me some remarkable morbid specimens. One of a tumour, of the size of two fists, removed from between the scapulæ by Mr. Vincent. A dark integument covering it, a suspicion arose that it was melanotic. The opinion was correct. Around the base, after removal, was found a multitude of other black deposits, which led us to believe that the disease would extend. The patient returned to the hospital with one tumour on the old cicatrix, and another near it. In this case the melanotic deposit was not confined to one organ. A large quantity was found in the chest. The lungs were compressed by surrounding melanotic matter. It also existed *in* the lungs. The liver contained several layers. In the mesentery were numerous depositions. The kidney, the two ovaries, in fact all the natural structure presented it,—the cellular tissue, the absorbent glands. It even pervaded the bones. The cancellous texture of the very vertebræ, you see, contains it. In this bottle is some of the woman's urine. It is dark-brown, or black, and at the bottom is a settlement of small gray melanotic matter, precisely like that of the tumour. A chemical examination of the blood will be made, and you shall know the results."

The lecturer then retired amid the general applause, with four or five of those neatly dressed friends who customarily attend introductory lectures, in the back ground of the area, to inspire the professor with confidence, prove their satisfaction of his competency, and divide the perplexing gaze of the audience. By these gentlemen he was warmly congratulated on his successful essay, in the ante-room, and in five minutes the audience were transported to the operation theatre, to see lithotomy performed.

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* It would be desirable to know something more of this fatal "surgical" disease in the hands of a physician. All the lecturers on surgery declaim against the division of surgery and medicine, though (with few exceptions) they oppose themselves to the only reform which can unite them in education. The present case might afford a convincing illustration of the propriety of enforcing the union after advising it.

October 12th.

Some clinical remarks were made today on the case of lithotomy just above mentioned. "Why, Gentlemen," said the lecturer, "did I perform lithotomy on this boy, instead of lithotrity, when the latter operation is often attended with so much less danger and pain? It is the duty of a surgeon to look at all the circumstances of a case of stone, because some of them will especially forbid one of these operations. By the by, the word *lithotomy* has been objected to for the cutting process, because it means cutting the *stone*. Lithotrity means *breaking the stone*, and that is right enough; but as the incision is made into the bladder, and not into the stone, the word *cystotomy* is more applicable to the operation. But what conditions are essential to justify the resort to *lithotrity*? First, a perfectly sound condition of the structure and functions of the bladder, which must not only be capacious enough to receive, but capable of retaining, five or six ounces of warm water, during the operation, to preserve the mucous membrane from injury, and enable us to seize the stone. The urethra, also, must be sound, and not irritable, so that the instruments may pass with comfort. Then the patient himself should be tractable, and his mind quiet and easy, or the crushing of the stone may be impeded by struggling. In young subjects there is the obstacle of a small bladder and urethra, and great irritability of the bladder, with difficulty to contain the necessary fluid. This renders lithotrity especially inapplicable to them. True it may be in some cases successful. I have seen Baron Heurteloup crush a stone in a child aged ten years, but that was one of the exceptions. On Saturday, the patient was a boy, but his character offered insurmountable objections to lithotrity. He was a most unmanageable lad. In fact, the surgeons in the country really sent him to town for the operation because they were afraid to attempt it on one who was so intractable. Nothing but force, and the most strictly applied bandages, would have availed for the crushing in his case. What mischief might occur to such a boy in the ten or twelve minutes of a lithotrity sitting. You saw, at the operation, that there was much difficulty in getting the staff into the bladder. His struggles caused this, partly, and partly his position. We generally introduce the staff first, you know, in cystotomy, and then bind the patient. But here we were obliged to bind first. On the steps of the operation I need not dwell, excepting to say that the incision was so made on entering the bladder (under the arch of the pubes, by a cut obliquely outwards and downwards), that it was unnecessary to enlarge it as the knife was drawn out again. A difficulty also occurred in extracting the stone, for there proved to be two, and the bladder contracted, as it sometimes will do on the object of search, requiring time for its release. After the operation I expected much constitutional derangement, and we had it,—high excitement of the nervous system, skin hot, a furred tongue, irritability of the whole alimentary canal, &c., for all which diet with broth and milk, warm baths, and hyd. c. creta, were ordered, and as inflammation of the peritoneal covering round the neck of the bladder was indicated, six leeches were applied. But he is now going on well. Respecting the stones, one was evidently separated from the other a long time before, a *spontaneous* splitting of the calculus, a rare event, but evidently the fact from the fitting surfaces of the two. Mr. Harding last week presented to the museum a large calculus taken from an adult, which had completely separated into its component parts, in the direction of its layers, spontaneously, in the bladder. I had never seen an instance of this, but Dr. Prout has since told me that it had happened in cases where he knew that no instrument had ever been introduced. So much for the case of stone. I shall next describe the section of the tendo-Achillis for club-foot, performed also last Saturday, in a child five or six years old, who had worn irons for a considerable time. The deformity presented inversion of the foot; the foot turned in, the outer and fibular side being directed downwards."

Here a porter entered, and spoke to the lecturer, who announced that an accident had just been brought in, which must be immediately seen, adding, "I can but occupy you usefully, here or elsewhere."

In a minute the theatre was empty,—and now for an illustration of the vicious effects of that want of punctuality which exists so generally in our hospitals, to the great disadvantage of the students. The hour appointed for the lecture was twelve o'clock, but twenty minutes had elapsed before the lecturer entered the theatre, and for twenty minutes the pupils were doing nothing on the benches. Twenty minutes do not seem so very long to keep students waiting once a week, because they have been used, session after session, to wait for lecturers in the theatres, or the squares of the hospitals, for three and four times twenty minutes beyond the appointed times; but it was quite long enough to deprive the students of nearly all the instruction which had been a *whole week* preparing for them; for in a quarter of an hour after the lecturer had commenced, the man carried him off, Mr. Stanley committing the mistake of presuming that he was going to occupy them as usefully in one place as in another. Suppose he had been punctual,—had entered the theatre at twelve instead of at twenty minutes past,—and that the supplementary benches, and the tray of casts, had been introduced five minutes before that time, so that *at twelve o'clock* the lecture

could have commenced. The clinical remarks on the club-foot could have been completed, and other useful information communicated which Mr. Stanley's share of the "vast multitude of in and out-patients of this great hospital" should have enabled him to supply. A *whole week* the students had to wait for it; but in the absence of that punctuality, about sufficient time only was obtained, after the introductory remarks, to remind the pupils of two practical facts, both communicable in twenty words, namely, that arteries should not be tied in *the dark*, and that the *character* of a patient should be studied before performing lithotrity. But surely Mr. Stanley went back to finish the lecture. No such thing. The examination of the new patient occupied fifteen minutes, and then, instead of inviting the hundred pupils to return to the medical theatre, he proceeded to "Pitcairn," "Lazarus," "Magdalen,"—a duty performed at a time which belonged wholly to the students and not to the patients, whom the surgeon could have visited as easily at three o'clock as at ten minutes to two. The result to the students was, that about a dozen followed him into the wards, for more than that number cannot do so profitably, and the rest of the audience idled about in the square, five and six in a group, or went to lunch, or returned to some study from which they were unsettled at twelve o'clock. It was painful to witness this wretched mismanagement and misappropriation of time.

An hour is a beggarly portion of time enough to devote to the clinique of an entire week. It ought to be two; nay, judiciously divided; one surgeon ought to occupy more. But if a full though stingy hour is to be appropriated to the "useful occupation" of the students, on the part of the teacher, at least that hour should be *useful*. But the time occupied in the operation theatre was not useful to any one. This fact might alone have induced Mr. Stanley to return to the lecture. The patient, a little boy, had fallen sixteen feet into an area, and fractured the skull in so many directions, without depression, as to render neither trephine nor saw available, so that nothing could be done, and the poor lad was removed "unavoidably to perish." That this consequence had followed a severe fall, was all that the crowd of pupils ascertained.

From this brief tale Mr. Stanley should draw a moral. Surely he will when the necessity is rendered so palpable. We do not give undue importance to the circumstances, for the resulting evil is of incessant occurrence, and imperatively demands reform, which this minute history of the mischief may possibly effect.

ON CIRRHOSIS OF THE LUNG.

By D. J. Corrigan, M.D.

This disease, in its symptoms and signs, bears the closest similarity to phthisis. Its formation exemplifies a law of diseased action, which extends through many organs of the body.

This disease is in the lung what cirrhosis is in the liver; and the author has, therefore, ventured to call it by the same name.* The name of cirrhosis has been given by Laennec to a peculiar disease of the liver, of which the pathological characters are a diminution in bulk of the organ, and its natural acini or lobules deposited in the meshes of a dense fibro-cellular web, the fibres of which radiate in every direction through the organ.

The disease of the lung, which is the subject of this paper, resembles in every respect this morbid change in the liver, modified, however, by the situation and differing nature of the organ. Like the liver, the lung, in addition to its interlobular cellular tissue, has, for the matrix of its vessels and air tubes, a cellular tissue, the fibres of which permeate the tissue of the lung in every direction. The lung, in addition to this, is enveloped in a fibrous or fibro-cellular envelope, which has been described by Dr. Stokes.

There is still another structure in the lung, the action of which assists the growth of the disease; it is the elastic tissue lying behind the muscular tissue of the trachea and bronchial tubes, and which, running in longitudinal fibres along these tubes, tends constantly to shorten them, and to draw all portions of the lung from the circumference towards its root. We see the action of this tissue in the living or dead body on an opening being made into the thorax, when we observe that the lung does not fall flat or collapse, but instantly, by the elastic action of this tissue, draws in its circumference in all directions towards its fixed point, the root. The structure of the lung, its fibrous covering, its great quantity of strong cellular tissue, and the elastic lining of its tubes, all tend to give it a liability to the process of contraction in its cellular tissue, which constitutes the

* A better name might be selected, but as there are already in medicine so many instances of names of diseases bearing no connection with their nature, and as the name of *cirrhosis* is universally retained for the analogous morbid action in the liver, although it only expresses an accidental quality of the disease, I have thought it better to retain the name, than burthen our nomenclature with another. I would rather add an additional fact than a new name to our science.

disease, which Dr. C. has ventured, from its analogy with a similar disease in the liver, to name cirrhosis of the lung.

As the process of diseased contraction goes on, the space that is abandoned by the receding lung cannot be thus filled up; the space that the contracting lung is deserting is therefore pressed in upon, or filled up in another way. In proportion as the contraction of the fibres of the fibro-cellular tissue obliterates the small air-vesicles, and as these contracting fibres, like so many elastic strings extending from the root in all directions, tend to contract or draw in the tissue of the lung, obliterating its small air-tubes and its blood-vessels, the large bronchial tubes dilate to supply the place thus left, until at last, when the disease has reached its last stage, the tissue of the lung, diminished to a very small size, presents no longer any permeable air-vesicles, but a dense fibro-cellular or fibro-cartilaginous tissue, with its fibres radiating in every direction through second and third sized bronchial tubes, dilated into cells or ending in culs de sac of every variety of size. In this state a section of the lung through these dilated and tortuous tubes resembles very much a section of those honey-combed stones which we find on the sea-shore, the work of the pholas. This dilatation of the bronchial tubes is partly owing to the contractile process going on in the tissue of the lung, partly to the expansive action of the parietes of the chest in the acts of inspiration. But as the contraction of the fibro-cellular tissue proceeds, other changes take place. The bony parietes of the chest begin to follow the receding lung, so that the side of the chest in which the diseased action is going on, falls in, and when the disease is in the right side, the heart follows the receding lung, so that instead of being felt in its natural position, it is gradually drawn over to the right side, and is felt beating under the right mamma. The liver is from the same cause drawn up remarkably high into the chest, and the left lung becomes greatly enlarged, passing over into the right side of the thorax, and sometimes attaining an increase of bulk of more than one-half of its natural size. When the left side is affected, the heart turns round until its apex is in a direct line under the axilla, or even still more posteriorly situated. The appearance of the lung necessarily varies according to the degree which the diseased contractile action has attained The lung presents much of its natural appearance, but it is denser and darker than natural, not brittle, but feeling to the finger like compressed or carnified lung, while radiating in every direction through it, are dense, firm, white fibres, which are the filaments of the fibro-cellular tissue becoming dense, firm, and white, by the contractile process going forward in it. The second and third-rate bronchial tubes, instead of growing smaller as they proceed in their course, are dilated, and end in culs de sac, while on the sides of these culs de sac, are seen the orifices of the still smaller branches; these smaller branches, on examination, are found to be impervious. The duration of this disease is generally slow, The same uncertainty of duration attends the analogous disease in the liver.

On the diagnosis of this disease, a few observations may be necessary. When the chest is examined for the first time, in a case where the disease is in the right lung, the displacement of the heart, and the prominence of the left side, might raise a suspicion that the fulness of the left side, and the displacement of the heart, were caused by effusion into the left side of the chest; but a little more careful examination serves to remove the doubt, and to show that the prominence of the left side is owing not to actual enlargement, but is only apparent, arising from the contraction which has been going on in the right side, while the clearness, on percussion, over the apparently prominent side, and the preservation of the natural form of the intercostal spaces, sufficiently prove that there is no effusion into the chest; and the natural sound of respiration equally proves that there is not pneumothorax of that side. But the disease, above all, that this affection most closely similates, is phthisis: in all the cases which have come under my own observation, the similarity has been most striking. In the onset of the disease, the attacks of hæmoptysis followed, or alternated by purulent expectoration: as the disease becomes more and more formed, the combination of these symptoms, and oppressed respiration, with dulness of percussion, bronchophony, and pectoriloquy, and the gargouillement in the dilated tubes, altogether imitate phthisis so closely, that the diagnosis becomes a matter of great nicety. The author scarcely doubts, that very many of those cases of phthisis which are recorded or noticed, as of very many years' standing, without any marked alteration for better or worse, are cases of this disease. In the commencement of the disease the surest diagnostic signs will be found in the absence of the constitutional signs of phthisis; and that if in a case resembling phthisis very closely, with hæmoptysis, bronchophony, &c., we do not find corresponding constitutional irritation, that the pulse is not disturbed, that there is not wasting, in proportion to the amount of local signs, we may infer that the disease under consideration is cirrhosis. There is, however, a circumstance relative to phthisis necessary to be recollected in making the diagnosis on these grounds; it is this, that if there be any other organic disease present in the case we are examining, the

absence of the constitutional symptoms of phthisis will not authorise us to infer that phthisis is not present. It is not at all unusual to see cases of phthisis supervening, on organic disease of the liver produced by intemperance, in which the same observation is applicable.

When cirrhosis of the lung is fully formed, and that we have the signs in full perfection, there is then no longer any danger of confounding it with phthisis; the very intensity of the local signs furnishes us with one of the diagnostic signs; for while the local signs, such as dullness on percussion, pectoriloquy, gargouillement, and absence of respiration, would seem to point out rapid and confirmed phthisis, the non-accordance of constitutional disturbance in correspondence with such local signs, tells us that it is not to tubercular disease those signs are to be referred. If the disease be in the right side, the displacement of the heart into the contracted side gives us at once positive evidence of the nature of the disease. In conjunction with these symptoms and signs, we have the history of the case, if it has been of long duration, to guide us; for while the present signs are those of phthisis, there is again a non-accordance between the history of the case and those signs; and while the signs indicate apparently confirmed phthisis, the history tells us that there has not been constitutional irritation or wasting corresponding with the local signs. One of the most curious circumstances connected with this disease, is the compensating power for the contraction of one lung, which is to be found in the proportional development of the opposite lung. In one case, the sound lung was increased by one-third, in another by one-half of its natural size. The slower the progress of the disease is, more certainly will this compensatory development take place. The time occupied before the contractile action has reached its last stage in the lung is very variable, just as it is in other parts where a similar action has taken place. We see this contractile action after burns, in strictures, &c., in cirrhosis of the liver, sometimes reach an extreme degree in a few weeks, while in other instances, years pass over with very little aggravation.

On the treatment of this disease the author has yet but few observations to make. The disease is very rare; for although it has been the subject of research for more than six years, he has been able to collect only a few cases. Pathological preparations of the disease are not uncommon; but these are of no use, without being able to connect them with the details of the cases during life. Dr. Corrigan thinks, that one principal indication is to obviate the attacks of intermittent congestion that every now and then occur, as the process of contraction acts upon the vessels, and indirectly upon the sound lung; these are characterised by febrile exacerbations resembling hectic, by attacks of hæmoptysis, or by aggravation of the cough, accompanied by viscid expectoration; they are at once relieved by small general, or by local bloodletting. In the intervals of these attacks, the relaxation consequent upon the profuse muco-purulent expectoration which often attends the disease, and the expectoration itself, are greatly relieved by decoct. senegæ, with sulphate of quinine in solution in it. The quietness of the pulse, the relaxation, and the profuse discharge from the bronchial tubes, would often seem, when the circumstances above noticed are present, to render it unadvisable to leech or bleed, but the patient bears moderate depletion very well. Whether we have any remedial agent that can exercise such a power as to check the contractile action in cirrhosis of the lung or liver, remains yet to be discovered. If the disease be, as analogy would lead us to suppose, of a low inflammatory nature in its origin, it might be judicious not to leave the cure of inflammatory affection of the lungs to tartar emetic alone, which is now so generally the practice, but to finish the treatment by the cautious exhibition of mercury.* Whatever will favour the development of the corresponding lung, so as to enable it to do its own duty, and also the office of its fellow, will, however, be the surest safeguard against the effects of the disease; and with this view, it is probable that the most active life, as to exertion, particularly in such exercises as will tend to develope the sound lung, such as long walking, riding, gymnastic exercises in moderation, will be the most beneficial habits for the patient; and above all, in the female, the discontinuance of the use of stays, or any dress that will press on the thorax and interfere with the compensatory development of the sound lung.—*Abridged from the Dublin Journal.*

* Dr. Stokes, in his recent work on Diseases of the Chest, has noticed, and is the first writer who has done so, that contraction of the side has sometimes followed the cure of pneumonia. Contraction of the side after pneumonia is not usual; and it is probable that, in the instance noticed, there was a deposition of lymph into the interlobular cellular tissue of the lung, which gradually took on the contractile action, constituting the disease here described.

SMALL-POX AND COW-POX.

To the Editor of THE LANCET.

SIR:—I have imagined that the following cases, which have occurred in my own practice, possess sufficient interest to be laid before the world; the observations

which accompany them are the results of my attention having been especially directed to this interesting subject. I am, Sir, your very obedient servant,

WILLIAM HARDING.

2, Charles-street, Soho-square,
Oct. 1838.

SECONDARY SMALL-POX, FATAL.

Elizabeth Palmer, a healthy but small child, seven years of age, was, in June, 1837, attacked with small-pox. She had never been vaccinated. There was nothing remarkable in the progress of the disease, which was of the distinct kind. The pustules were tolerably numerous, circumscribed, and separated from each other; the fever subsided on the appearance of the eruption, and was not renewed; the pustules maturated in the usual way, and the general health of the patient seemed to be little affected after the subsidence of the primary variolous fever. During her convalescence four children of a Mrs. Chelly, residing in the same house, were all attacked with small-pox (they had never been vaccinated), and all suffered severely; the disease being, in each case, more or less confluent. Immediately afterwards the child of a lodger in the same house suffered a severe attack, and recovered. Ten weeks from her first attack, the child Palmer complained of headach, and appeared drowsy; she huddled to the fire, shivered, and felt chilly; in the evening she complained of pain in the epigastrium, and vomited considerably. She passed a restless night, complained next day of headach, and appeared languid and oppressed; on the same evening an eruption was to be detected on the forehead and chest, and a slight efflorescence on several parts of the body, and on the third day the eruption was general. Throughout the progress of the disease the brain appeared remarkably torpid, and she answered questions, as it were, after an effort to collect a simple idea. The face and head swelled enormously at an early period of the disease, so that no feature could be distinguished; and the vesicles on the face seemed to come forward in one continuous flattened bleb. On the trunk and extremities the eruption was partly confluent, partly distinct, but pale and flaccid, almost without areola. When the pustules broke the fœtor was almost insupportably filthy; a dark saliva was poured forth from the mouth, the child breathed with difficulty, and on the tenth night died, having passed through two attacks of small-pox in about twelve weeks; the first a mild but well-marked case of variola discreta, communicating the confluent and coherent disease to other children in the house; the second a malignant attack from its commencement, accompanied by symptoms of a low typhoid character.

SMALL-POX AND COW-POX ADVANCING TOGETHER, — VACCINE INFLUENCE PRECEDING VARIOLA.—SMALL-POX SINGULARLY MODIFIED.

Susan Thebles, a fair, fat, healthy, laughing girl, from Essex, was, in May last, in service near Soho-square, in a family where small-pox prevailed; she was not in attendance on the sick, but frequently passed into the room. After some days she mentioned that she had never been vaccinated, and being in attendance, I immediately procured lymph, and made several pricks in each arm; on the fourth day after the vaccination she complained of considerable headach, retired early to bed, vomited, and passed a restless night. On the following day the febrile symptoms were very great, the eyes were red and glaring; skin excessively hot; headach intense; the light was painful, and there was constant vomiting of bilious matter. Fifteen leeches were applied to the head; the patient kept quite cool, and supplied with ice-water. The next evening she was better, and on the following day fancied herself able to rise and resume her duties. Through all this constitutional disturbance the vaccine vesicles progressed beautifully, and appeared very perfect, excepting that the radius of the areola was smaller than what is generally observed. In this case two pimples were detected on each arm, which, in their progress, appearance, and decline, presented all the characters of distinct small-pox; and the nurse, who carefully examined the body, informed me that four pustules were to be seen on the right thigh.

SMALL-POX AND COW-POX ADVANCING TOGETHER.—VARIOLA PRECEDING VACCINE INFLUENCE.—SMALL-POX NOT MODIFIED.

In the summer of 1836 Mrs. P—— requested me to vaccinate her child, as the small-pox was at that time in the house; I did so, and two or three days afterwards the child sickened for small-pox. It had a severe attack, was covered by the eruption, and the face remains much marked. The vaccine disease seemed to run its usual course, preserving its own character; and on the right arm, there being no small-pox pustule within a short distance of the vesicle, the inflamed circle was well delineated.

Remarks.—In detailing the particulars of cases which have occurred in my own practice, and which I have considered worthy of record, from the anomaly of their characters, I do not put them forth as insulated facts,—as things differing from what has been before observed. Secondary cases of small-pox, particularly where the first attack has been produced by inoculation, and cases of small-pox and cow-pox advancing together

each vesicle preserving its own character, are occasionally observed; but fatal cases of secondary small-pox, both occurring on the same spot, observed by the same persons, without any particular atmospheric change having taken place, and when the disease was not raging epidemically, are so rare and so conclusive, that I should consider my duty ill fulfilled, if the facts were not offered to the world. Nor can the occasional occurrence of such cases be said to invalidate that remarked peculiarity in the exanthemata, "that they take place but once in the same individual during life;" it only shows that no law in disease is so universal as to be without exception; nor does the history of this single case interfere with the truth of the generally-received opinion, "that the difference of small-pox does not depend upon any difference of the contagion," because we have daily proof that the confluent produces the distinct, the distinct the malignant, &c.; yet, to no other cause can I refer the severity of the second attack; for several weeks, in several of the apartments of a house filled with people and furniture, the severest forms of small-pox prevailed; it may, consequently, be said to have been saturated with variolous poison. Now, it is legitimate to conclude, that the first attack of variola had, agreeably to its customary action, imparted to the constitution of the child, E. P., a certain power of resistance, but that power was imperfect, inasmuch as it was overcome by the continued concentrated influence of a virulent contagion; hence the severity of the second attack and its fatal results.

The case of Susan Thebles is, I think, a striking instance of the influence of vaccination; the severity of an attack of small-pox, that is, of the eruptive stage, is generally in proportion to the severity of the irritative fever; but in this case, though the precursory symptoms were almost unusually violent, they subsided after three days, were never renewed, and only about eight pustules made their appearance on the body. The third case simply shows vaccine and variolous diseases advancing together, the latter somewhat preceding the former, and neither being influenced or acted upon by the other, but each preserving its own or separate existence.

Having made these remarks on small-pox, I will intrude a little further to say, that I met, a few months since, with several mild or modified cases of small-pox, occurring after vaccination; and in my own establishment a very severe case, where the vaccine cicatrix was perfect and well defined. I also lately made several attempts to revaccinate, but in no case did I succeed where the evidence was satisfactory that the disease had had a prior existence in the system. On my own person an experiment was made at the Vaccine Institution, where I exhibited a circular, well-defined, pale, indented cicatrix, twenty-five years old, and requested, if possible, that the disease might be reproduced. Five pricks were made in the left arm, with a round-pointed lancet, charged from a ten-day pock, the 51st remove from the cow, and the parts were kept covered some time with the lymph. Five pricks were then made in the right arm, from an eight-day pock, the 49th remove from the cow; and the same extra care was taken, that plenty of lymph should penetrate the wounds. In eight hours each puncture assumed much the appearance of urticaria. In twenty-four hours there was a circle of active inflammation around each, and on the third day a minute quantity of matter on the summit. Now, a non-professional person might, I think, have easily mistaken this for a case of secondary cow-pox, though, of course, it had none of the characters of the true vaccine vesicle. On the whole I am inclined to think secondary cow-pox, after perfect vaccination, to be as rare as secondary small-pox after inoculation, and when it does occur it must be referred to idiosyncrasy, like secondary cases of the other specific fevers.

The cases of modified small-pox that I have met with by no means favour the absurd notion, that the protecting influence of vaccination wears itself out, after a seven, ten, or twenty years lease, for they have principally occurred in children. The true explanation of modified small-pox is, I believe, imperfect vaccination. Where the vesicle gets, by some accident, prematurely broken, or when the system is not in a favourable condition for receiving vaccine influence, in such cases the protecting power of vaccination holds good in proportion to the perfection of the vaccine vesicle and the constitution is left liable to receive small-pox, when exposed to variolous contagion, which disease is altered or modified in proportion to the extent of saturation which the system has received from the imperfect vaccination.

Severe or confluent small-pox, where a perfect cicatrix can be seen, is extremely rare, and must be referred to a strong natural disposition to the disease, or to that peculiarity of constitution which renders some persons liable to receive some of the exanthemata two, or even three times, unless it be possible for the disease to be locally perfect without constitutional effects; that is, is it possible for a vaccine vesicle to progress regularly, the system being, at the same time, engaged by some general functional derangement, arising from dentition, ascarides, mesenteric affections, &c. &c.? and may, under such circumstances, a perfect cicatrix be produced, without the system having had imparted to it any power of resisting variolous contagion?

THE LANCET.

London, Saturday, October 20, 1838.

The death of Mary Ann Strong shortly after her removal, under suspicious circumstances, from *Grove House*, Southampton, affords a singular illustration of the inefficacy of the present system of public inspection. The country is put to considerable expense; seven or eight thousand pounds a year are expended on Commissioners and Inspectors; the asylums are taxed heavily for the express purpose, yet there is too much reason to believe that the helpless, imprisoned lunatics are far from being protected, or treated with that humane consideration which their dreadful situation demands. They are shut up from the public view; their friends, too, often abandon them; and the keepers of asylums, interested in the continuance of their insanity, take the roughest and readiest ways of restraining the exhibition of their unruly passions. There are bright examples to the contrary, but restraint, personal violence, and cruel chastisement, in the worst spirit of the old flogging pedagogue, are too often appealed to, while quiet, gentler agencies, and means calculated to turn the eddying currents of the mind into the right channels, are forsaken as too tedious and too troublesome.

As we stated a short time ago, the certificate of two medical men is sufficient to consign any man to the madhouse; and the example of Sir Anthony Carlisle, last year President of the College of Surgeons, although it may now be viewed as a beacon, shows how easily a man may be thrown into the ranks of the insane. Few cases so glaring as *that* ever occur; but when it is considered that the question often is not whether a patient is insane, or a monomaniac, but whether he should be confined in a madhouse or allowed the moderate exercise of his will in society, grave apprehensions arise, particularly as the great majority of medical students are excluded from the public asylums, and thus expressly prevented from studying mental diseases. For reasons well known to the Governors and Officers of Bethlem, and other lunatic asylums, the public only obtain admission on *one fixed hour* in the week, and medical practitioners, as well as students, are refused access at any other time. That the wretched patients may be placed at the absolute mercy of the keepers—that such disgusting and heart-thrilling scenes may be enacted as Parliamentary Inquiries dragged to light at Bethlem and York, and other places—the medical student is deprived of all opportunities of acquiring a clear knowledge of insanity. What is the consequence of these arrangements? Not only are lunatics ill treated and rendered incurable, but hundreds are under confinement who ought to be free. The liberation of Tom—accidentally leading to a melancholy result in Kent—called down severe comments upon the conduct of Lord John Russell; but is it not infinitely more reprehensible to detain persons in confinement who ought to be at liberty? Yet this is done with impunity.

To return to the case at Grove-place. This institution, situated in Southamptonshire, is kept by Mrs. H. Middleton. We perceive, by a Parliamentary Return, that she held it in 1830 in conjunction with Mrs. Ansley; that it was licensed to hold 31 males and 31 females; and that in the year preceding 19 patients were discharged, while 6 died. Mary Ann Strong, who had been confined in the Asylum, was removed by her friends in consequence of certain reports; she died shortly afterwards. The circumstances connected with her removal and death induced the husband to apply to the Coroner, who instituted an inquest, which continued five days. The deceased was a pauper, aged 49 years; she was sent to Grove House on June 11th, 1838, and removed September the 28th, having remained there rather more than three months. The Guardians of the Poor paid 6s. a-week to the establishment. Death occurred five

days after the patient's return home. We proceed to give an abstract of the evidence from the "Hampshire Independent:"—

Frances Sturdy deposed that she was sent for from Newport, on that day week, (the 29th of September) to attend on Mrs. Strong, the deceased. Found many bruises upon her person, particularly her eye and collar-bone. Several bruises were on her legs. Her back, from her neck down to her loins, was quite black. She was violent at times, not constantly. Witness was with her from the time just mentioned until she died. Her death took place on the following Thursday. Her violence consisted in her talking and crying out; she had no waistcoat on when witness came; it was put on the deceased on Sunday morning; she wanted to go down stairs, and to get out of the window; that made it necessary to subject her to the waistcoat; no medical attendance was procured during the week witness was there; she (witness) did not think it necessary; she had been in the habit of attending upon insane persons some years ago, but not latterly; the marks she had referred to were in spots, not in stripes, as if inflicted with a stick; the front of the body and legs exhibited these spots; the bruise was under the eye; her eye and neck were much swollen; the spots in front of the body were inflamed; they did not extend below the ribs; when witness washed her on Sunday she said, "I hope you are not going to wash me with a birch-broom, and throw a pail of water over me."

Different witnesses deposed that the injuries were not inflicted in conveying Mrs. STRONG home, nor subsequently. Mr. C. FOWLER examined the body after death. The manner in which Mr. FOWLER gave his evidence is instructive. Technical terms should never be used on these occasions when the idea can be expressed with *precision* in common language.

Preparatory to going into the evidence of this witness in reference to the *post-mortem* examination of the deceased, some discussion took place in consequence of his avowal that the notes made on that occasion were taken down by another hand, at his dictation, and that they had since been supplied to him. It was consequently held that such a document would be inadmissible as evidence, if the proceeding should be carried into another court, and his testimony was therefore taken from his resolution (?) of appearances presented by the body in question. He is a medical practitioner in this town. He did not know Mary Ann Strong during her life. Made a *post-mortem* examination of the deceased on Friday last. There were many round spots, of various sizes, and of a dusty brown colour, scattered over the superior and inferior extremities (I mean the arms, legs, and thighs), as well as one spot of about two inches or two inches and a-half in length, immediately under the left collar-bone, and running parallel to its axis; there was also another large spot at the upper and outer part of the thigh, of the same colour as those before described; the back was discoloured over every part of which pressure had not been applied; those discolourations were very black, and around its margin was seen an efflorescent appearance of the skin. [At the request of Mr. Randall, Mr. Fowler explained to the jury that bright red was meant by efflorescent.] Just behind the calf of the left leg were found two red narrow lines, resembling scratches; he saw no particular appearance on any part of the face, nor under the eye; the darker-coloured appearance on the back he believed to arise from the gravitation of the blood after death, and consequent upon death; did not attribute the whole of the discolouration of that part of the body to the same cause. He could not ascribe any probable cause to the remainder. It arose from external causes before death. As a medical man, he could judge whether the discolouration was caused before or after death. In his judgment the discolouration on the back, to which he could assign no direct cause, was the result of violence.

Could a person have inflicted the discolouration in question by his or her own act?—He might.

Are you enabled to state any means by which such an injury could be inflicted?—I cannot. The appearances on the collar-bone were produced by injuries previous to death. The marks on the thigh were caused before death—were caused by the external application of force. I should call marks so caused bruises. The lighter-coloured marks on the collar-bone were, in my judgement, produced by external violence; so were all the other marks, except the deeper portion of the discolouration of the back, which, as I before stated, was occasioned by the gravitation of blood after death. There was no swelling whatever on the back—no hardness to be felt under the skin. I believe the discolouration arose from some of the vessels being torn, under the skin. There was a swelling under the skin of the bruise below the collar-bone. There was a similar swelling on the upper and inner part of the right thigh. In my judgment none of the external injuries of which I have spoken were likely to have accelerated death. I believe they were not likely to have produced internal injury which would have accelerated death; as to whether they would produce mental irritability would depend upon the cause which produced them; if they had taken place from blows inflicted by

others, they would have produced mental irritability. I examined the internal parts of the body, also the internal parts of the head. On opening the chest, a strong membranous band, the result of inflammation, was found between the membranes attached to the membranes of the lungs, and to the reflected membrane of the internal surface of the walls of the chest. The right ventricle of the heart was dilated, and its walls very thin. The intestines, especially the larger ones, were greatly distended. The vessels of the external membrane of the brain were much congested on the left side. Saw opacity, the result of inflammation, of the middle membrane of the brain, in the right side of the superior longitudinal sinus; beneath the membrane in this part was found about a teaspoonful of colourless fluid; more fluid than usual seen in the lateral ventricle.

Mr. FOWLER's facts were not affected by the cross-examination.

The question to be determined was the cause of the lunatic's death; whether it had been the immediate or indirect result of violence. Dr. HENRY CLARK was examined at considerable length on this point; but his opinions, although on the whole just, and judiciously stated, are of less importance here than the facts. He, as surgeon to the Union, visited Mrs. STRONG. "I found her," he says, "decidely insane; I reported to that effect to the Guardians, and that she was in such a state that I considered she could not safely be left in the care of her friends; she was sent to Grove-place Asylum by order of the Guardians."

Did you see Mary Ann Strong during the time she was at Grove-place?—Yes, I saw her twice in my quarterly visits.

How long after she had been placed there was the first time you saw her?—I think about six weeks.

Did she appear better or worse than before she went there?—I could not see any material difference.

How long from that time was the second occasion of your seeing her?—Nearly three months.

Can you say at what time that was?—It was on the Saturday preceding the last week in September.

Was she better then or worse?—Worse; much worse.

In what state was her bodily health?—I think she appeared more emaciated, but I could not judge correctly, as she had a strait-waistcoat on.

Did you see her alive after that period?—I did not, nor have I seen the body.

By Mr. Patterson.—You have stated that it was the 5th of May when the deceased was sent to the Asylum?—I am quite sure I signed the certificate on that day, but did not mean to say for certain that she was removed on that day.

Do you know, of your own knowledge, on what day she went to the Asylum?—I only know that I gave the certificate, and that she was to have gone on that day.

This affords an example of the responsible duties that devolve on the parochial medical officers; their fiat sends the pauper to the lunatic asylum, and it is a part of their duty to visit him under confinement. By the way, it appears that Dr. CLARK signed the certificate on *May 5th*, and that the patient was not sent to Grove House until *June 11th*. Now, if this pauper, labouring under acute insanity, complicated, too, with visceral disease, was not seen for a space of *five weeks*, the inference appears inevitable that the salary only enables the medical officer to supply the sick-poor of the Union with very scanty attendance. It was attempted, in the defence, to prove, that Mrs. STRONG required croton oil to act on the bowels, and that her death was owing to constipation, and the neglect of purgatives during the five days she was at home. How, then, if the neglect of five days was fatal at the close, had she passed five weeks in the earlier stage of the disease without medical attendance? Again, Dr. CLARK only visited Mrs. STRONG twice at Grove House, and says, in his evidence, that "once a quarter are the periods for medical visits to lunatic asylums." Dr. CLARK confounds the visits of the Union officer with the inspection of the visiting Justices and Dr. OKE. The 9th Geo. IV. c. 40, s. 40, expressly declares, that while confined in an hospital, "medical practitioners, appointed by parishes," may visit their lunatics *eight times* in the course of the year; and, surely, a visit every six weeks would not be too frequent; the case of Mrs. STRONG is an instance of the evil that may be inflicted in a shorter interval. It must, however, be admitted, that the inadequate salaries paid at present

to the medical officers of Unions, place the efficient performance of their duty to the sick-poor out of the question.

The visiting physician, Dr. OKE, and the Justices by whom he is appointed and paid, inspected Grove House four times a year; Dr. CLARK saw the paupers quarterly; yet what was the treatment Mrs. STRONG received. The circumstantial evidence is too faithful an interpretation of the bruises upon the body. Two females were examined who had been inmates of Grove House at the same time as Mrs. STRONG. The futile technical objections to their evidence can only weigh with lawyers; one—SOPHIA WILLIS—was intrusted with the keys of the outer gate, and had charge of Mrs. STRONG and other patients.

Sophia Willis.—I had charge of Mrs. Strong until I left the establishment. I slept in the same room with her, but not in the same bed. There were five other females in the same room, besides myself and Mrs. Strong. It was rather a large room. At times Mrs. Strong was very much afflicted. She was incapable of leaving her room, but I dressed her, on account of her highly deranged state. We got up at about seven o'clock in the morning. I went down stairs into the yard where the other females were. There were about 32 females in the establishment when I left. The yard is open to the air. It has no covering. It is gravelled under foot, and walled round. The door was open leading into the yard. I could go into the building from the yard. Mrs. Rose was the nurse. I cannot say at what exact time they breakfasted, as there was neither clock nor watch, but should think it was about eight o'clock. There are two rooms —one for the very violent, and one for those who were not so. The patients are washed sometimes. I have seen Mrs. Strong washed. I don't recollect whether I have seen Mrs. Rose wash Mrs. Strong or not. There is an assistant nurse, Caroline Sellens, who is the daughter of Mrs. Rose: I have seen her wash the deceased. She had water thrown over her when quite undressed; she was standing in the middle of the yard at the time, entirely naked. The water was thrown from a bucket, which was about half full. I don't recollect that more than one quantity was thrown. Mrs. Strong was not afterwards rubbed dry; Sellens did not use a towel nor anything else for that purpose. Mrs. Strong was kept standing naked there about a quarter of an hour. There were other females in the yard at the time; I cannot say how many, but, as it was a place resorted to by all, I suppose most of them were there. There was no other naked at the time. After she had remained there for the time I have mentioned, she went into the straw-house, which is in the yard. There is straw in it, and cribs (sleeping places), with loose straw at the sides. This house is boarded and covered. It has a room over the lower apartment. I do not recollect that there were any other females in the straw-house at that time. Mrs. Strong had no towel given to her when there. I do not know that she was sufficiently conscious to rub herself. I did not go in with her. I do not recollect seeing her come out. I have seen her washed at other times in a room, but not in the open air. She was on those occasions quite naked, and washed with cold water and a flannel. This occurred frequently. She was sometimes struck, sometimes struck very hard, by Mrs. Rose and her daughter. I have seen a stick used upon her many times a-day. That occurred every day, and more than once or twice in every day. Different sticks were used; any that came to hand. I think I have seen a stick used thicker than my finger, but one as thick as my finger was generally employed. They had sticks about the house for the purpose. I never saw a whip or a broom used on Mrs. Strong. I have seen her beaten when she was quite naked. She was driven down stairs with a stick from her bed-room into the yard, quite naked, where cold water was thrown over her, and afterwards she was driven into the straw-house in that state; it was Caroline Sellens who drove her and struck her several times; she repeated the blows all the way down stairs, and again repeated them in the yard; she was quite naked all the time, and the blows were inflicted chiefly upon her back; this particular beating occurred about two months before I left the Asylum, as nearly as I can recollect; I have seen Mrs. Strong beaten after the time I have been describing; it was continued almost daily until the time that I left; I do not recollect that I ever witnessed any other treatment of Mrs. Strong that ought not to be done; I do not recollect seeing any one else beat her; it was with a small stick that she was then beaten; she continued to sleep in the same room with me; I saw the marks left by the beating about her back; they were red at first, but afterwards turned black. Neither Mrs. Rose nor Caroline Sellens used me ill. The surgeon and other officers of the establishment used Mrs. Strong kindly as far as I know; none of them used me unkindly.

Mrs. FANNY SMITH, a widow, who had also been an inmate of Grove House, was sworn. She had made shirts, caps, and bed-curtains at the Asylum; and though irritable, her memory appears to have been sound and faithful.

Mrs. Smith knew Mrs. Strong at the Asylum. Witness slept in the same room with her, but not every night. Witness always slept there, but Mrs. Strong was about two nights in a week, and sometimes three, absent; she meant this was the case in some particular weeks; Mrs. Strong had slept for an entire week there; seven slept in that room; it was not a very large room; had seen Mrs. Strong in the yard; had known of persons coming to visit her; heard Mrs. Rose tell her that some one had come to see her; could not say on what day of the week that was, but knew it was on a Sunday when Mrs. Shakell came; Mrs. Rose's daughter said it was Mrs. Shakell, and recollects something being done to Mrs. Strong on that day—on the Sunday; when they (Mrs. Strong and Sellens) came into the yard, she (Sellens, the under nurse) slapped Mrs. Strong's face on both sides; she did this with her open hands, both together, more than once; thinks three times; Sellens told Mrs. Strong that she should be stripped stark naked, and sent to bed without supper, for "telling the lies to Mrs. Shakell;" that evening witness saw her stripped naked and sent up without supper. Witness saw that, and "heard the screeches after she was up stairs." Witness had seen Mrs. Strong washed in a very indecent manner. She was stripped naked, and scrubbed with a broom in the open yard, where she might be seen by any one from the top of the castle (an adjacent building). Nearly all the patients were in the yard at the time. There were windows in the castle looking into the yard. There were two windows looked into the yard from the Asylum. Witness had nothing to do with the castle, and did not know who occupied the apartments. Had never seen men look out of the castle windows, but had seen men on the top of the castle. Mrs. Rose was present at the washing of Mrs. Strong, and Caroline Sellens was not far off. She was in sight. Mrs. Strong was naked, and washed with cold water and rubbed down with a broom. Could not exactly say whether she was wiped dry afterwards or not. Believed not. Could not say how long she remained in the yard that time. It might be for half-an-hour. She was naked the whole of the time. Saw her leave the yard. She went into what was called the straw-house. She did not go there of her own accord. She was put there by Mrs. Rose and the girl. Had seen her beaten many times. Mrs. Rose had taken witness's stick (a heavy crutch stick) from her hands, about three inches thick, to strike Mrs. Strong. Had seen Mrs. Rose strike Mrs. Strong violently with that stick (holding it up to the Court). She struck her on the back and head, legs, arms, on any other part of the body. Had seen her also struck with a fagot-stick at the straw-house. Witness had seen one as large as her stick. There was a small switchy one, which was worse than the larger ones, because so many little branches were about it. Had seen Caroline Sellens strike Mrs. Strong many a time worse than her mother. Had seen Mrs. Strong *struck many times with a stick on the head;* could not say how often. *Those beatings took place every day, and many times a day.* On one occasion she made a noise; on another she had refused to take her dinner; could not say that she was always beaten when she made a noise, but when Mrs. Rose was present she was. Had seen her head washed, and seen her head pushed down into a bucket of water, to teaze her and perplex her. The witness was here again cautioned against volunteering opinions. It was cold water; it was done by Mrs. Rose and her daughter.

By the Jury.—She was kept all night in the straw-house; witness heard so. It was a birch-broom, worn to the stump, that she was scrubbed with. Witness never mentioned it to the surgeon; if she had she should have been served so herself. When at work with Mrs. Rose she never dared to speak to her about this treatment; she should have been knocked down if she had. She knew she should have been knocked down, because she had seen others served so on such occasions.

By the Coroner.—She did not dare to mention the treatment inflicted on Mrs. Strong, because she (witness) would have been served the same; she would have been stripped naked, washed, and turned into the straw-house.

By the Jury.—Witness meant by knocking down, they would lay hold of her, and throw her down. Had seen the face, nose, and mouth bleed of those who had been so treated. Had seen Mrs. Strong used in that manner. Had seen her cut with a stick, and then thrown violently down.

The evidence of Mrs. SHAKELL, to whom SMITH refers, shows how easily the keepers of asylums can evade detection; at the same time it corroborates the statements of the preceding witnesses.

Mrs. Elizabeth Shakell deposed as follows:—I have known Mrs. Strong for thirty years. I visited her at Grove-place; the the last time was on Tuesday, the 16th of September; I did not go alone; Mrs. Cardy, from the Isle of Wight, accompanied me; it was nearly three o'clock in the afternoon when I reached Grove-place; we did not see her immediately on our arrival there. I expressed a desire to see Mrs. Strong, but there was an interval between that request of a quarter of an hour or 20 minutes before Mrs. Strong was introduced; she was brought to us in the parlour; she did not speak at first, but cried very much; she knew Mrs. Cardy; she called me

Betsey, and addressed Mrs. Cardy by her right name; she asked Mrs. Cardy for her daughter; Mrs. Cardy has a daughter; she said, "I was naked but a short time before I was here brought to you;" she put up her foot and said, "These are not my boots, I wear a chain;" there was a girl present at the time, the girl who brought her into the room; I do not know her name; she appeared to be about 16 or 17 years of age. Mrs. Strong spoke about gipsies; she said she was to be turned into the road that night amongst the gipsies; the female in question observed, that she had merely told her that because she made a noise; I observed the girl colour very much when speaking on the subject; Mrs. Strong looked very weak and ill; I had known her a long while before she was mentally afflicted; she was not of a strong constitution; she was about 49 years of age; her appearance, when I saw her at Grove-place, was most untidy; her clothes were scarcely put on at all.

To come to the cause of death. Mr. Fowler found adhesion of the pleura, and dilatation of the *right ventricle* of the heart. The pleurisy was, apparently, not recent, and if it had, to that extent, could not have been fatal. It is much to be regretted that Mr. Fowler did not measure the size of the heart; this was the place for technical precision, as Bouillaud, Clendinning, and Bizot have accurately determined the size of the heart in health and disease. Dilatation of the heart is a vague term, and is not very frequently found on the *right* side,—still dilatation might have really existed. The membranes of the brain were apparently inflamed, and feculent accumulations existed in the bowels. What shall we say to the treatment of this patient at Grove-House? Would the bitter contumely, the aspersion of water on the skin, the evaporation from the unwiped surface, the scrubbing-broom, the scourge and club mercilessly applied, the blows on the head and spine, and the cold, straw crib, cure the pleurisy, calm the dilated heart, or allay the action of that excited, inflamed brain? The surgeon of the Asylum was all the while administering croton oil twice a week. Under these circumstances would the scourging be likely to accelerate death in one so weakly, so worn down, and labouring under such a severe cerebral affection? Need we answer? *Flogging* has killed the strongest men. We must remark here, that one of Dr. Clark's answers is exceedingly reprehensible. He was asked whether insane persons would not bear exposure to cold to a much greater extent, without injury, than those who are not afflicted with that malady? His answer was, "Certainly they do."

Then, if an insane person were exposed in the way I have described, would it not be much less productive of injury than to a sane person?—It would.

That insane persons are freely exposed to cold, and other exciting causes of disease, is too certain, but that it is without injury we deny. The contrary is a most dangerous doctrine, and supported neither by figures nor facts. It is a loose conjecture, and a mere excuse invented by the keepers of asylums, to justify culpable negligence.

Insanity, far from being a disease unattended by danger, is more fatal in its results than the ordinary diseases which fall under medical treatment. Because a few madmen have lived to a great age, some medical men have been led to look upon "insanity "as not necessarily prejudicial to longe-"vity." Direct observation proves that a more flagrant error never existed.

"*Mortality of Lunatics in Asylums.*

"Bethlem, St. Luke's, Stafford, York, Lincoln, Gloucester, and Hanwell—lunatics:—Total patients treated, 19,782; living one year, 22,295; deaths, 2057; annual deaths out of 100 living, 9.02; deaths out of 100 treated, 10.40.

"General population, age 40—45, Sweden (1811—1830):—Annual deaths out of 100 living, 1.50.

"The annual mortality among lunatics was 9 per cent.; the annual mortality of the Swedish population, at the age of 40—45, was 1.50 per cent. It need scarcely be added, that at this age the mortality of the Swedes differs inconsiderably from that of other European nations; *madness, therefore, increases the mortality sixfold.* The mean age of lunatics does not *exceed* 40—45 years. The mean ages of 977 patients admitted in five years at Bethlem Hospital, were, in 1830, 37 years; in 1831, 35; in 1832, 37; in 1833, 36; in 1834, 36; so that 40—45 may be safely taken as representing the ages of the entire class—those above,

as well as those below that central point."*

The mortality increases with age (p. 13); and, although it is lower in females than males, we can scarcely be astonished at the death of a lunatic aged 49. Still, the treatment in the present case was eminently calculated to accelerate that event; the verdict of the jury was, therefore, just.

The jury, after deliberating for about two hours, unanimously returned the following verdict:—"The jury are of opinion, that the death of the said Mary Ann Strong was occasioned by natural causes, accelerated by the ill-treatment she experienced from the two nurses, Mrs. Rose and Catherine Sellens, during her confinement in Grove-place Lunatic Asylum."

Mrs. Rose, who appears to be *all thorn*, and her daughter Sellens, who had been taught so young to torture the mad, have been discharged by the proprietors of Grove-House; this was right; and the question now comes, should not Mrs. Middleton herself be treated in an analogous manner when she next applies to the magistrates for a licence. Mrs. Middleton knew that a straw-house existed; and she culpably neglected the supervision of the inmates committed to her charge. This is explicitly established by the evidence of Willis:—

A juror.—Did Mrs. Middleton, the proprietress of the establishment, ever come to see the patients, particularly the female patients?

Witness.—No, I never saw Mrs. Middleton.

This question the Coroner required to be put several times, and it was repeated accordingly, and elicited precisely the same answer.

We have long entertained doubts of the capacity of *females*, or, indeed, of any person who has not received a medical education, to conduct lunatic asylums. According to the present Acts of Parliament, if a house contain 100 patients, there must be a *resident* physician, surgeon, or apothecary; and why should 70 not be as carefully watched as 100?

It is quite clear, from Dr. Oke's evidence, that the inspection of the visiting physician and the magistrates is a mere empty formality; the Roses and their thorny progeny have sufficient time to lay aside their scourges, and call up the patients in review before the easy, *gentlemanly* inspectors. The medical officer is present to explain any anomaly. This cannot last much longer,—the entire system must be re-organised; Mr. Du Bois, and his sleepy commission in the metropolis, must be aroused from their lethargy; the Chancery lunatics must be carefully visited; the magistrates must be relieved of a duty which they cannot perform efficiently; and the whole management must be duly consolidated. All these reforms will follow in the wake of a *National Faculty of Medicine*.

In the meantime the principle of *assurance* can alone secure the humane, efficient, restorative treatment of lunatics. On this point we take a passage from the pamphlet before cited (pp. 39, 40):—

"To make one class of people take the same care of another as they take of themselves, it is necessary to make their interests identical. This community of interests must not only exist, but be apparent almost at first sight to a mind of ordinary intelligence. It should, if possible, appear more intimate than it is. Benevolence, justice, all the higher feelings, are engaged in favour of lunatics; it is, therefore, only necessary to neutralise the opposite interests of the keepers to insure their treatment in the manner best calculated, according to our present views, to promote recovery and prolong life. The average rate of mortality and recovery is the index of the external circumstances to which lunatics are exposed. Confinement, suffering, bad food, uncleanliness, accelerate the lunatic's death, and prevent recovery. Proper medicine, kind treatment, moderate exercise, healthy, varied food, employed judiciously, tend to advance recovery and to preserve life.

"For insuring the preservation of life and animal comforts, the ancient practice of giving a *natural idiot* to people who enjoyed his estate so long as he lived, was a masterpiece of legislation. Your idiot, in such circumstances, was treated with care and kindness; he was a privileged fool, sauntering about, or basking in all the pleasures which his half-developed soul could participate. He was not locked up in the dark keep and dungeon; he was not left to freeze

* "On the Statistics of English Lunatic Asylums, and the Reform of their Public Management," by Wm. Farr, pp. 12, 13.

in chains; he was not tortured by a savage keeper, because the instant he died the estate passed from the hands of the patron to the heirs of the idiot.

"As regards congenital idiots, with large fortunes, no better system could be adopted now; with this modification, that only a part of the annual income need be appropriated to this purpose. The idiot should be placed with a person who had received a medical education: some prefer placing him on the same conditions with a relative, not the next heir.

"In lunacy another element must be taken into account: the persons under whose care the lunatic is placed, must be made to have a direct interest in his life, and in his *recovery*. This will be effected by making them *assure the lunatic's life and recovery to a variable amount, depending on the sum paid annually for his maintenance and treatment*. The statistical facts already collected, prove that it is practicable. This will be rendered more evident by two or three examples.

"At Bethlem, 6 persons die, 46 recover in *one year*, out of 100 admitted; if, therefore, *a pound* were deposited with each person at the time of admission, on condition that 16*l*. 13*s*. should be paid to the relatives in case of death, this would leave a small surplus, and the interest, in the hands of the governors. For 100 persons would deposit 100*l*.; on an average 6 of them would die, and their friends would receive 6 sums of 16*l*. 13*s*. each, in all 99*l*. 18*s*. Observe how, in this way, the managers of the hospital would be interested in the lives of the patients. If 5 instead of 6 died, they would gain 16*l*. 13*s*.; if 7 instead of 6, they would lose 16*l*. 13*s*., a difference the slowest understanding could perceive. Again, if the governors deposited 1*l*. in the hands of the patient's friends, on condition of receiving back 2*l*. 3*s*. 6*d*. if he recovered during the year, they would be repaid; for 46 recover, and 46 times 2*l*. 3*s*. 6*d*. = 100*l*., the amount deposited by 100 patients. If only 23 recovered, they would lose 50*l*.; if 69 recovered, they would gain 50*l*. They would be intimately interested in the patient's recovery. Without any deposit on either hand, then it would only be necessary that the managers of the hospital should pay 16*l*. 13*s*. when a lunatic died, and receive 2*l*. 3*s*. 6*d*. when he recovered, to insure the greatest efforts possible on their parts to effect recovery and preserve life. This was the problem to be solved. The *medical officers and keepers* should, in the same way, have an interest in the patient's *life and recovery*.

Chemistry of Organic Bodies. — Vegetables. By THOMAS THOMSON, M.D. J. B. Bailliere, London, 1838. 8vo., pp. 1076.

WHILE in France and Germany the chemistry of vegetable bodies has, within the last few years, made rapid progress, it has received but few contributions from the labours of British chemists. The subject, nevertheless, presents a wide and inviting field for scientific research. The resources of the vegetable kingdom are far from being exhausted, and there is every reason to hope that the diligence and perseverance of the distinguished men, who are now engaged on the ultimate analyses of vegetable substances, will accelerate in no small degree the advancement of medicine, by placing new and powerful agents in the hands of the practitioner. We are, therefore, highly indebted to Dr. Thomson for having undertaken and accomplished the laborious task of exhibiting a complete view of the present state of vegetable chemistry. It is impossible for us to attempt anything like an analysis of his work, which contains more than one thousand pages of matter in large octavo. We must content ourselves with saying that each division of the subject, and each substance, is treated in the most careful and complete manner. In a word, the "Chemistry of Vegetable Bodies" will long continue to form a standard treatise of the highest scientific order. At a time when the medical press of this and other countries is engaged in the production of so much that is futile and utterly worthless, it gives us sincere pleasure to direct attention to an English work of which the author and the country may be justly proud.

Flora Medica; a Botanical Account of all the more important Plants used in Medicine, in different Parts of the World. By JOHN LINDLEY. Longman and Co., London, 1838. 8vo. pp. 655.

THIS also is one of the works the details of which it is impossible for us to examine, in order to justify the strong manner in which we feel bound to recommend it to the medical public. The object of Mr. Lindley's "Flora Medica" is described in a few words; it is to lay before the botanical student a correct, systematical description of the various plants which are employed medicinally in this and in foreign countries.

The necessity of such a work is the more evident, that the student has hitherto been compelled to derive his acquaintance with the species of medicinal plants, chiefly from works treating of the British Flora, or to rely upon authorities which are far from being "infallible." Thus, with respect to this latter point, Mr. Lindley acquaints us, in his preface, that one writer describes cubebs as being obtained from Sierra Leone, where the piper cubeba does not grow. Again, rhubarb has been said, by different writers, each having equal confidence in his opinion, to be derived from the root of rheum palmatum, rheum undulatum, and rheum emodi. Finally, it has been stated, upon competent authority, that of the few vegetable drugs admitted into the last edition of the Pharmacopœia of the College of Physicians, twelve are referred to plants which *certainly* do not produce them, while twenty-six others have been assigned to their sources with more or less inaccuracy.

The above, and many other examples, which we might enumerate, show how important it is that the investigation of medical plants should be taken up by some one fully qualified for the proper execution of so difficult a task. No one was better adapted to fulfil the object which he has professed than Mr. Lindley, and we feel confident that his work will obtain the patronage which it justly merits.

IMPERFORATE ANUS.

THE RECTUM TERMINATING IN THE BLADDER.

To the Editor of THE LANCET.

SIR:—I have taken the liberty of transmitting to you the following case of imperforate anus, which has lately occurred under my observation, and which, from its rarity, may perhaps be sufficiently interesting to find a corner in your valuable Journal. I remain, Sir, your humble servant,

W. C. RANDOLPH.

Hungerford, Berks, Oct. 8, 1838.

Sept. 24, at 5 A.M., M. B. was delivered of a fine male child, which, upon examination, was found destitute of every appearance of an anus, nor was the skin between the scrotum and coccyx found to be elevated in the slightest degree, that, and the surrounding surface, being entirely on a level. In consequence of the mother's repugnance the operation commonly resorted to in these cases was not performed, the child being consequently left *ad naturæ curum*. In the after part of the day, after continued efforts in straining, a small quantity of meconium was observed to pass off through the penis, which naturally led to the conclusion, that the rectum terminated in the bladder. The child refused the breast when offered, nor could be prevailed upon to take it until the following morning, after which it became frequently sick. He again, after great straining, passed off a very small portion of meconium, which he continued to do twice daily up to the day of death. In a few days the abdomen became swollen, tense, and painful, the contortions of the intestines being very plainly perceived. The child gradually wasted, and continued to do so until the 3rd of October, when, at 1 P.M., after receiving the breast (which it had not done previously for three days), he was seized with convulsions, which lasted several minutes. The child gradually revived, and continued, apparently, in great pain until the following morning, when, at 5 A.M., he was seized with a second fit of convulsions, in which he died.

Upon examination post-mortem, the whole of the smaller intestines, and the upper part of the colon, were found to be distended with flatus; the lower part of the latter, and the whole of the rectum with meconium. Upon tracing the rectum downwards, it was found to continue in its natural course as far as the bladder, into which it opened immediately under and posterior to the prostate gland, and by passing through a probe the opening was found to be nearly of the same dimensions as it would have been if natural.

THE LECTURES ON MEDICAL JURISPRUDENCE AT ST. GEORGE'S HOSPITAL.

To the Editor of THE LANCET.

SIR:—In the last No. of THE LANCET, it is correctly stated, that the present lecturers on medical jurisprudence at St. George's Hospital are Dr. Robert Lee and Mr. C. F. Macauley, a young barrister; but the additional statement, that,—"A medical gentleman was first suggested as the coadjutor of Dr. Lee, but he declined that gentleman's services on the ground of his being a *general practitioner*," must, I think, be incorrect.

As I am the individual referred to in the above paragraph, permit me, in justice to Dr. Lee, from whom I have never received other than kind and gentlemanly treatment, to explain why it appears to me an impossibility that he should have been the raiser of the objection referred to.

In a conversation which passed between us, at Dr. Lee's own house, upon the subject of the vacant lectureship, Dr. L., mentioned that he had volunteered his own services, in conjunction with those of any other respectable person, as the future lecturer on medical jurisprudence, upon which I in-

quired whether he would object to be associated with me, in the event of my being chosen to fill the vacancy. His answer was promptly, distinctly, and unequivocally expressed in the negative; and, at subsequent, as at previous interviews, when the subject was referred to, he always declared it to be his wish that I might be the chosen person.

In justice, also, to a gentleman whom I have never seen, and, personally know nothing of, will you further allow me to add that Mr. Macauley, the young barrister alluded to, is reported to me to have gone through a complete course of medical studies before being called to the bar—and has, therefore, the right to be esteemed a man of medical science, as well as a professional lawyer. His general talents are spoken of very favourably; and he is also said to possess, in common with his distinguished brother, considerable eloquence. The union of all these qualifications, will, I hope, render the lectures on medical jurisprudence at St. George's Hospital alike interesting and profitable to those who are now compelled to attend them. If the interests of the students are better taken care of by the preference awarded to this gentleman, I shall certainly not regret my being the rejected candidate. I have the honour to be, Sir, your obedient, humble servant,

William Barrett Marshall, R.N.

6, Cheyne-row, Chelsea, Oct. 6, 1838.

MEDICAL SOCIETY OF LONDON.

Monday, October 8th, 1838.

Mr. Bryant, President.

Mr. Robarts read a paper

ON THE TREATMENT OF NEURALGIC AFFECTIONS BY THE LIQUOR OF YELLOW BARK, AS PREPARED BY MR. BATTLEY.

The object of this paper, in brief, seemed to be a recommendation of the above preparation as superior to every other form in which bark can be administered, in reference, however, chiefly to neuralgic affections. The author conceived that it exerted a more powerful influence than quinine in these painful disorders, inasmuch as it contained all the principles of the bark in a highly concentrated form. It was a more stimulating tonic than quinine, and, by its effect on the stomach, seemed to enable the system to counteract the disease. The author had used the remedy in three cases of neuralgia. The first case was one in which a man, 37 years of age, had been subject to a pain in his head, which came on every night at bed-time. His general health was in every other respect good. The disorder had occurred at the above periods for three weeks. He took ten minims of the liquor cinchonæ three times a-day; in two days he was well. He continued the remedy for a week, and the pain did not return. In the second case, a young woman had suffered from severe pain in the fingers of the left hand, coming on at first periodically at night, but afterwards affecting her almost every hour during the day. She had been suffering for nine months, and had been under various physicians, who had prescribed various remedies; it was more than probable that quinine and bark itself had been given. With the exception of the neuralgia she suffered from nothing but want of strength. Eight minims of the liquor cinchonæ, four times a-day, were administered. In a week the pains had ceased. She continued the remedy a fortnight afterwards; the pains did not return. These cases only showed the effect of the liquor cinchonæ in neuralgia; the next case would show its superiority over other medicines in this disorder. The patient was a lady, 67 years of age, and in December last suffered from shooting pains in the right side of the head. During the first month they were constant, occurring at intervals of a minute or two, and preventing her from sleeping; there was also slight numbness of the right thumb, but no headach, vertigo, or tinnitus aurium. She was treated antiphlogistically, and got well in a few days. On the 17th of February the pain again came on, but now commenced at the sixth cervical vertebra, and extended down both arms. Blisters, aperients, mercury, bleeding, anodyne liniments, were tried with the effect of increasing the pain. A belladonna plaster afforded temporary relief only. Cupping, followed by a seton in the neck, aggravated the complaint. No permanent good resulted from large doses of carbonate of iron combined with quinine, or from quinine with belladonna, or from counter-irritating remedies. Physician after physician had been consulted. At length, by the advice of Mr. Battley, the author tried the liquor cinchonæ. It was administered in doses of ten drops, four times a-day, and the pain, which now (25th of June) had for some months been periodic, did not come on till three hours after the usual time; the dose was gradually increased till it reached twenty minims, and in a few days all pain disappeared, and has not returned since. In cases which were not neuralgic the author did not find the liquor cinchonæ superior to quinine.

Dr. J. Johnson said, that from the great uncertainty in the duration and other characters of neuralgia, it was difficult to say when it was cured. It would sometimes suddenly cease of itself after the trial of a great number of remedies, the last getting all the credit. One, two, or even twenty cases, were not sufficient to establish the success of any remedy. The liquor cinchonæ, like other remedies, might occasionally be of service, but it would often be un-

successful. How were we to know that this preparation contained all the qualities of the bark? The formula for its production was kept secret, and we had only the *ipse dixit* of one man, that it did contain all these properties. Until it was shown that the preparation could be made by other persons, we had no proof that it contained all the properties of the bark. In the first and second cases related the pains were periodic, and arsenic or quinine would have effected a cure. In the third case the disease seemed to have worn itself out, terminating suddenly, as neuralgia sometimes will. The *post hoc*, therefore *propter hoc*, was one of the most fallacious axioms in medicine.

Mr. CRISP said no conclusion could be drawn from the cases related. Instead of three or four cases a thousand, he should say, were necessary to arrive at a definite conclusion respecting the virtues of a medicine. He had always found quinine successful in the treatment of periodic neuralgia. The fixing of the value of remedies from a few isolated cases tended to mislead us respecting the powers of medicines.

Mr. PROCTER had found that the neuralgic cases he had had under his care during the last year were not genuine tic douloureux; they were of a mixed character, partly neuralgic and partly rheumatic. Equalising the circulation he found the best treatment in these cases.

Dr. L. STEWART thought the fact of the gradual increase in the severity of the symptoms, in the third case, and their gradual decrease under the use of the liquor cinchonæ, were evidence in favour of the powers of that remedy.

Mr. DENDY had always found the quinine the most powerful preparation of bark in periodic diseases.

Mr. PILCHER believed that Mr. Battley's preparation was merely a highly concentrated solution of bark in cold water. Dr. Dickson had informed him (Mr. P.) that quinine did not produce the specific influence that bark effected.

Dr. JOHNSON had always found quinine more effective in periodic diseases than bark in substance, or any of its preparations. He would not believe that the liquor cinchonæ was merely a watery infusion; he believed it impossible that such a minute quantity of water could take up all such active principles of bark. Until Mr. Battley proved it to be a mere watery solution, he (Dr. J.) would not believe it to be so if he swore to it.

Monday October 15th, 1838.

Mr. BRYANT, President.

WHAT IS THE LIQUOR CINCHONÆ?

IN answer to a question Mr. ROBARTS said he understood the liquor cinchonæ flavæ was prepared by Mr. Battley in the same way as the liquor opii sedativus; it was merely a concentrated watery solution. In the same way were prepared the liquor belladonna, sennæ, &c.

Dr. SHEARMAN had found that the following mixture was exceedingly serviceable in cases where the stomach was so weak as not to bear other preparations of bark:—Powder of yellow bark, two drachms; magnesia, one drachm; water, eight ounces; filter. Two tablespoonsful three times daily.

SEA-SICKNESS,—WHAT IS ITS CAUSE, AND WHAT WILL PREVENT IT?

Dr. WHITING called the attention of the society to the subject of sea-sickness. He had lately returned from Ostend, and had a very rough passage, and consequently suffered much from sea-sickness. Though he had most carefully observed the various symptoms of this peculiar state, he had not been able to satisfy himself as to the physical cause of the phenomena. He thought it would be a subject of much interest to the Society to enter into a discussion on the subject. Regarding the symptoms of sea-sickness, as experienced by himself, he first suffered from a feeling of nausea like that produced by an emetic, and this was increased by every dive of the ship; a great degree of stupidness also soon came on. This was followed by excessive exhaustion, which continued during four or five hours. As the motion of the ship became less, the sickness also decreased, one seeming to depend entirely on the other.* Various causes of sea-sickness had been suggested by writers. One, he believed Dr. Wollaston, considered that it was dependant on the fact of the circulation being incapable of following the motion of the ship. Another had assigned the continual motion of the contents of the stomach as the cause. Whilst a third had believed it was dependant on impressions conveyed to the brain through the medium of the eye. He considered that the second cause would not hold good, inasmuch as many persons suffered distressing sickness whose stomachs had not received food for a long period.

Dr. SHEARMAN said, that as blind men were sick at sea, it could not be considered that the last cause mentioned by Dr. Whiting had much effect in the production of the sickness.

The PRESIDENT had known two instances of blind persons suffering much from sea-sickness.

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* Byron appears to have noticed the sickness as dependent on the *motion* of the ship entirely. In his admirable description of the shipwreck, in the second canto of "Don Juan," Juan becomes first affected as "the ship gave a lurch," and it would appear from the context, that the sickness and the rising and sinking of the ship, were contemporaneous.—REP. LANCET

Mr. Crisp considered that sea-sickness was mainly attributable to the motion of the vessel. Some persons were unable to ride with their backs to the horses, in a coach, without experiencing sickness, and the motion produced by this cause was analogous to that of a ship in a rough sea. He thought that this motion produced a peculiar effect on the brain, and that the stomach was secondarily affected, in a manner somewhat similar to what is observed in cases where blows on the head produce irritation of the stomach. He did not consider the sight had anything to do with it. Regarding the prevention of sickness at sea, he had tried various means for effecting the object,—lemon juice, laudanum, starvation, repletion,—all had failed.

Mr. Headland believed that sea-sickness depended chiefly on a peculiar impression produced on the brain through the medium of the eye, subsequently affecting the system in general. It was, in fact, an inability in the affected person to maintain the sense of equilibrium.

Mr. Dendy thought that members only took a partial view of the subject. The senses, no doubt, had often much to do with the matter; but, certainly, not always; persons had been awaked from sleep by the occurrence of the sickness. Fear was not always present; the most courageous persons frequently suffering from sickness. It was well known that Nelson, who "never knew fear," was incapable, for many days, of leaving his berth from sickness, and Sir John Franklin was never able to take charge of his ship until he had passed the Bay of Biscay. He (Mr. D.) believed that the *cause* of sea-sickness existed, primarily, in the heart, an irregular action being produced in that organ, like that arising from concussion of the brain. The consequence was, that the brain did not receive a sufficient supply of the circulating fluid, and the phenomena of sea-sickness were produced. This was proved to be the fact, also, from the circumstance of the recumbent position, in some measure, preventing the sickness, the circulation in the brain being then more powerful. In syncope we placed persons in the recumbent posture to obtain this latter result. He believed that the circulation was primarily affected, but the subject of sea-sickness was involved in great obscurity.

Mr. Leese thought that the sickness arose from some impression made on the brain, and probably through the medium of the circulation. He remembered an instance in which some old seamen, part of a ship's crew, after rowing in a small boat for several hours, became very sick. These men were never sick when exposed to the motion of the ship, to which they were habituated.

Dr. Shearman knew the case of a young man who had made two voyages to India, and who had never conquered the disposition to sea-sickness.

Dr. Whiting thought that the sudden pitching of the vessel must have considerable influence in deranging the circulation, which, affecting the brain, the stomach also became disordered. Secondary causes came in to hasten or to increase the effects.

At the suggestion of Mr. Crisp, it was agreed that the subject for the next meeting should be a consideration of those diseases which are benefitted or increased by a sea voyage.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

ON THE MODUS OPERANDI OF NITRATE OF SILVER AS A CAUSTIC AND THERAPEUTIC AGENT.

By Dr. R. D. Thomson.

The importance of nitrate of silver to the surgeon for the purpose of destroying excrescences upon the human body, is sufficiently well known, but the mode in which it produces the object desired has, perhaps, not been attended to, nor has it been made a subject of investigation. It is usual to recommend excrescences to be *destroyed* by caustic; but this term would imply that the surgeon had it in his power to destroy matter, whereas physical science demonstrates to us that we can merely change the condition of matter. To determine the exact effect produced by the action of nitrate of silver, when employed as a caustic, was the object of the author in undertaking the experiments which formed the subject of this communication. When nitrate of silver is applied to the cuticle, if its operation is carefully watched, it will be observed that there is first a white precipitate formed, or that the cuticle becomes white. This proceeds from the formation of chloride of silver, from the common salt contained in the sweat. But, if the caustic is allowed to remain on the cuticle and is not neutralised by ammonia, which dissolves the precipitate formed, the residual undecomposed nitrate of silver gradually penetrates into the organic matter and decomposes it; or, in other words, forms chemical compounds with the gelatin or modification of albumen of which the cuticle consists. The decomposed matter, when examined, is found to consist of two compounds; one of these is insoluble in water and may be collected on a filter, washed and dried; the other is soluble in water, and passes through the filter; it is procured by evaporating the washing of the first precipitate; hence the mode in which nitrate of silver removes an excrescence. We require to moisten the caustic before we can expect to produce any consi-

derable effect, because chemical action is always accelerated and promoted by solution, although the old proverb is not true that "corpora non agunt nisi soluta." On the surface of the skin there is a continual supply of fluid to a certain extent from the natural excretion, and therefore the action of nitrate of silver would, at first sight, appear to take effect without the interposition of fluid. Nitrate of silver, therefore, as a caustic, removes the part to which it is applied by decomposing it, and this decomposition is a chemical action which occurs between the organic matter and the metallic salt. This result may be illustrated on a large scale by decomposing the white of egg by means of nitrate of silver. White of egg is usually considered to be albumen, but this is a mistake, as that substance is a heterogeneous mixture of different matters, upon which no precise experiments have been made, so that all the analyses and galvanic experiments with white of egg are of no value, as they have not been made upon albumen alone, but upon a variety of substances. White of egg may, therefore, be readily taken as affording an example of mixed animal matter. When a solution of this substance is poured into a solution of pure crystals of nitrate of silver which have not been in contact with organic matter, a white precipitation immediately ensues, which gradually becomes darker from the upper surface downwards. At first sight it greatly resembles chloride of silver, but on being agitated or stirred with a glass rod, it has a congulated aspect, approaching to the consistence of false membranes, being stringy, and presenting the appearance of of incipient organisation. When carefully observed the white of egg is thrown down by nitrate of silver in the form of white flakes, which, viewed through a glass vessel, have a curdy appearance, but when poured from one vessel to another, resemble thick custard. After being allowed to stand for some time, that portion of the precipitate next to the light changes from white to pale violet. When thrown on a filter, the external layer of precipitate becomes brown, and latterly darker; but if the precipitate be cut into it is found to be white as the original white of egg. It would appear to differ from chloride of silver in the circumstance that, under a microscope, the latter never appears white. A very dilute drop of a solution of nitrate of silver was placed on the object-glass; a small portion of common salt was then added,; instantly a great number of little brownish corpuscles, too minute to be discriminated in reference to their form, made their appearance. Upon agitating the fluid they began to adhere to each other in elongated masses, and to simulate the appearance of electrical clouds, with long ragged arms. After many trials, it was impossible to detect the precipitate of a white colour, even although to the naked eye it presented this aspect. Again, the chloride of silver, when filtered, allows no deposit to fall, saving that of the oxide of silver. But, on the contrary, when the compound of white of egg and nitrate of silver is thrown on a filter, the fluid which passes through yields, by evaporation, a dark compound, probably a basic salt. So that we have two compounds formed, one of which is soluble, and the other insoluble in water.

Now, these experiments, which we can perform upon dead matter, constitute an epitome of what takes place when nitrate of silver is brought in contact with the internal living tissues; the mucous membrane of the intestinal canal is constantly pouring out a fluid which contains animal matter in solution, resembling the white of egg. In the experiments already detailed, the nitrate of silver when applied to this fluid decomposes it in the manner described, viz., two compounds at least are formed with the solid matter which is in solution in the animal fluid, one of which may be dissolved by any water which may come in contact with it, and the other is insoluble in that menstruum; so that the quantity rendered soluble will depend upon the state of the stomach, in reference to the quantity of fluid which it contains. If the quantity of nitrate of silver be greater than is sufficient merely to decompose the mucous secretion, the metallic salt penetrates to the membrane itself, and forms a compound with the animal matter of which it consists, or produces what is called in common language disorganisation. We learn from this very word the dangers of improper nomenclature, for it has thrown a veil in the present instance over one of the simplest processes, a process which is a constant occurrence in the laboratory, and yet so blinded have medical men been by obscure notions about vital action, nervous energy, irritation, mesmerism, and other expressions, of which the meaning is not properly understood, that they have overlooked entirely the secondary causes of the action of remedies. Now it is by studying those secondary causes, in the first instance, that we can alone expect to mount higher. We discover the existence of a tree in the forest by its lofty branches: the detection of the nature of its root is an after investigation. When still stronger doses of nitrate of silver have been administered, the salt penetrates to the blood-vessels, forms compounds with their coats, or disorganises them, makes its way into the blood, acts upon the solid in solution in that fluid, and produces a chemical compound which, in plain language, we designate by the generic title of a coagulum. It is in this way that hæmorrhage is stopped. No other method of stopping hæmorrhage is known than by shutting up

a vessel by means of a plug. When we tie an artery we cannot succeed in uniting the opposite sides permanently unless we cut through the internal coat; that is, unless we allow the vessels to meet and throw out their nutritive matter into a common cavity, when it becomes an organised coagulum.

NOTTINGHAM BRANCH OF THE BRITISH MEDICAL ASSOCIATION.

We are glad to perceive, from the following document, that the practitioners of the midland and northern counties are at length becoming sensible of the real position which they should assume in order to forward the all-important object of medical reform. We trust that the example which has been given by Nottingham, and some other localities, will be speedily and extensively followed throughout the kingdom.

To Geo. Webster, Esq., M.D., President of the British Medical Association.

Sir:—I have the pleasure to inform you that at a numerous meeting of the medical practitioners of this town and neighbourhood, held September 26th, 1838, in the Assembly Rooms here, Dr. Mitchell Davidson in the chair, the following resolutions were agreed to. I am, Sir, your obedient servant,

HENRY C. ATTENBURROW, Hon. Sec.
Castle-gate, Nottingham,
Oct. 11, 1838.

That the present state of the medical profession requires its members to unite, in order to defend and promote their rights, interests, and respectability, and to cherish kind and honourable feelings towards each other.

That a Branch of the British Medical Association for Nottingham, Nottinghamshire, and the adjoining counties, be now established.

Thereon an amendment was proposed—

That an Association of legally qualified medical practitioners for Nottingham, Nottinghamshire, and the adjoining counties, to be designated the Midland Counties Medical Association, be now formed.

The original motion was carried.

That a president, six vice-presidents, a treasurer and secretary, be annually appointed.

That eight committee-men be chosen, four of whom shall retire by lot at the end of the first year, and subsequently four others be elected along with the other officers, who together shall constitute a Committee of Management,—four to be a quorum.

That the Committee of Management forthwith draw up a code of rules, in conformity with the laws of the Parent Association.

OFFICERS.

President.—Dr. J. M. Davidson.

Vice-Presidents.

Mr. Wm. Wright,	Dr. Hutchinson,
Dr. Howitt,	Mr. Greeves.
Mr. Cooper (Mansfield),	

Treasurer.—Mr. Robert Davison.

Hon. Secretary.—Mr. H. C. Attenburrow.

Committee.

Mr. G. Mills White,	Mr. Orton (Beeston),
Mr. Isaac Massey,	Mr. Burrows,
Mr. Billings,	Mr. Valentine (Sutton in Ashfield).
Mr. J. N. Thompson,	

That this Branch Association do suggest to the Parent Association the propriety of appropriating not less than two-thirds of the sum transmitted from each Branch Association towards the formation of the Benevolent Fund.

(Signed) J. MITCHELL DAVIDSON, M.D., Chairman.

Dr. Davidson having left the chair, it was resolved unanimously,—That the thanks of this meeting be given to Dr. Davidson, for his able and impartial conduct in the chair.

LATIN AT THE HALL.

(*From a Correspondent.*)

To convey some idea of the knowledge of Latin possessed by the Examiners at Rhubarb Hall, the following *facts* may be mentioned:—One of these sages went to the table where sat a youthful candidate for the licence, and said to him, "You will begin, Sir, at *imprimis* (long, instead of pronouncing the *i* short), and end at (*so* and *so*)."

Presently afterwards he said, "Perhaps, Sir, you will have the kindness to parse a line or two."

Cand. Anywhere, Sir?

Ex. Of course. Here, if you like.

Candidate parsed a line.

Ex. What tense is *sit?*

Cand. The conjunctive mood.

Ex. Very well, Sir, *I see you understand your Latin well.*

To another candidate, whose schedule the Examiner was signing, the learned apothecary said, "It is true, Sir, that you have got through your *examination*, but you have not done with *the Latin* yet, and I would, therefore, strongly recommend you to *keep it up* (the dignified conversational style of the old ladies is, in the highest degree, becoming their office and position), for you will have to pass another examination, where they will be *much more strict!!*"

Cand. Of course, Sir, I intend to brush up my Latin.

There are hundreds of medical students, "fresh and green from the country," who

dread the Latin examination quite as much as the examination for the licence,—not with reason, from its severity, but from the ignorance of the secret tribunalists before whom they have to appear. What a figure would the worshipful Board cut, if they were obliged to conduct their examinations in public! No wonder that they and their scribes deprecate so strongly the important reform which is demanded in this respect.

PUZZLING DIFFERENCES IN RESULTS.

The following fact manifests the care which is required in conducting scientific investigations, and the caution with which peremptory opinions should be pronounced in the early stages of an inquiry. Caoutchouc, or "Indian rubber," cannot be dissolved, either in water, alcohol, any acid, or any alkali, but it is soluble in *ether*. Macquer, in 1767, pointed out this latter fact, which Berniard afterwards attempted to put to the test, subsequently declaring, that Macquer had erred, for that caoutchouc was scarcely soluble at all in sulphuric ether (which Macquer had used), and that even nitric ether did not perfectly dissolve it. This difference of announcement was singular, each chemist being remarkable for powers of observation and a character for accuracy, and his acquaintance with the subject examined. But Mr. Cavallo discovered that the truth apparently belonged to both; that Macquer was quite right, while Berniard was not wholly wrong. He found that ether, when *newly prepared*, seldom or never dissolved caoutchouc completely, but that if the ether was first washed in water, it dissolved caoutchouc with facility. The washing of ether, says Dr. Thomson in his "Chemistry of Organic Bodies," deprives the ether of a little alcohol, with which it is often impregnated, and adds to it a tenth of water. Alcohol will precipitate caoutchouc from this solution.

NEW TINCTURE.

The absence of education among many of the dispensing chemists frequently leads to awful mistakes. It is not of so much consequence when jalap is simply mistaken for rhubarb, but when laudanum is replaced for syrup of poppies, or stavesacre sold for jalap, and oxalic acid for Epsom salts, the error is more momentous. Sometimes the blunder is only laughable. A "chemist" had lately a prescription to prepare, in which, after a certain infusion was ordered, there came the words, "Tincturæ ejusdem." But he happened not to have a morsel of "Ejusdem" in the house, either in tincture, powder, or infusion, though his shop was filled with bottles and drawers, and fitted up as fine as a gin palace. He therefore promptly sent to Messrs. Godfrey and Cooke for a small quantity of "Tincturæ Ejusdem," not doubting that every tincture in the world was to be had there, whether "*that same*," as Pat would say, or some other.

Abdominal Inflammation during Pregnancy.—Mr. Wilkinson, of Aspatria, informs us, that since his recent communication was made to The Lancet, in which he pointed out the advantage and utility of employing mercury, and the safety of administering cathartics in abdominal inflammation occurring during pregnancy, he has treated a similar case in a woman not pregnant, where the mucous membrane of the bowels was involved, and the disease yielded to copious depletion, fomentations, warm oily enemata, and a combination of calomel, opium, and ipecac. in small doses, frequently repeated. The disease quickly subsided as soon as the mouth became affected with the mercury, and the woman has quite recovered.

Want of Reform in the Coroner's Court. — Mr. E. W. Wilkinson, surgeon, was summoned to a village distant four miles from his residence in Aspatria, about nine o'clock in the morning of September 22nd, to see a coal-miner, who was said to have been severely burned by the ignition of the inflammable gas, while in the mine, and who died on the following morning. The Coroner was accordingly sent for to hold an inquest, but he *never came*, and the unfortunate young man was interred without the observance of that form. On the 28th of July, also, he was summoned to attend a married woman belonging to the same village, who was said to have fallen backwards from a horse, and to have been taken up by her husband in a state of complete insensibility, in which she continued till about eight o'clock on the next morning (Sunday), when she died. The Coroner held his inquest on the Monday, when the jury returned a verdict of "accidental death," and imposed a deodand on the horse of one shilling. Although a surgeon attended this woman, and treated her as labouring under severe concussion, and more or less effusion of the brain, yet the Coroner did not request either his attendance or that of any other medical man at the inquest.

TO CORRESPONDENTS.

The public are indebted to Mr. Cheere, one of the Governors of the Westminster Hospital, for the resolution that was carried at the meeting, reported in The Lancet of Saturday last, page 128. The name of the mover was misprinted.